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ORIGINAL ARTICLES.

PRINCIPLES UNDERLYING THE SERUM DIAGNOSIS OF TYPHOID FEVER AND THE METHODS OF ITS APPLICATION.

Presented in Opening the Discussion on Serum Diagnosis in the Section on Practice of Medicine, at the Forty-eighth Annual Meeting of the American Medical Association, at Philadelphia, Pa., June 1-4, 1897.

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I comply the more readily with the suggestion of the Chairman of this Section that my remarks in opening this discussion on the "Serum Diagnosis of Typhoid Fever," shall relate to the general principles of the method, inasmuch as the results obtained by this method at the Johns Hopkins Hospital will be presented in the course of this discussion by Dr. Block, and others will relate the results of their personal experience.

Before the introduction of the Widal method of diagnosis the discovery and subsequent studies of the typhoid bacillus had been comparatively barren of results available to the general practitioner of medicine. In this respect the typhoid bacillus afforded a marked contrast to many other pathogenic microörganisms, notably the tubercle bacillus, the diphtheria bacillus and the malarial parasite.

For various reasons improved methods of diagnosis of typhoid fever are most welcome to the practitioner. Inasmuch as the prevalence of this disease is a just reproach to the sanitary conditions of a locality, there has often been a readily explicable, if not creditable, reluctance on the part of some physicians, especially in public institutions, country towns and summer resorts, to make the diagnosis of typhoid fever even in clear cases. Moreover, notwithstanding all that has been written on the subject, knowledge of the frequent deviations of typhoid fever from the classical type, amounting sometimes to entire absence of all features of this type, can hardly be said to have become sufficiently common property of the medical profession in this country. The differential diagnosis of typhoid fever from certain other diseases, such as acute miliary tuberculosis, tuberculous peritonitis and meningitis, acute ulcerative endocarditis and various other septic affections, may, for a time at least, be most difficult or impossible, even to skilled diagnosticians. In children the disease is prone to anomalous manifestations. Since the discovery of the malarial parasite there is no longer any excuse for confounding typhoid and malarial fevers. A method of positive diagnosis of typhoid fever will elucidate the much disputed nature of many short and mild febrile diseases and of certain fevers of warm climates, as in the southern part of this country.

Before the introduction of serum diagnosis, numerous attempts had been made to utilize the presence of the typhoid bacillus for purpose of diagnosis. One can obtain with considerable regularity cultures of the typhoid bacillus by hypodermic puncture of the spleen in typhoid patients; but this procedure is not wholly without danger. Anyone who has seen at autopsy a swollen, soft typhoid spleen with its capsule distended to the utmost and ready to burst the moment it is lifted from the body, would certainly hesitate to insert even a hypodermic needle into such a spleen during life. Instances are on record in which such puncture of the spleen has given rise to severe intraperitoneal hemorrhage. Cultures from roseola spots, suggested by Neuhauss in 1886, yield uncertain results. Cultures from the blood give positive results in some cases, especially if large amounts be used. Cultures from typhoid stools reveal the presence of the specific bacillus in many cases if the examination be made with sufficient patience and care. The introduction of the Elsner and of the Capaldi nutritive media marks a distinct advance in this method. None of these procedures, however, in their present form afford simple and ready methods of diagnosis in the routine of hospital and private practice.

History.—The Widal method of diagnosis is based on the application of scientific discoveries made before Widal's first publication, June 26, 1896. As a controversy for priority, attended with no little bitterness, has arisen, it may be well to state the main historic facts. Like so many other bacteriologic discoveries of practical utility, this one is the outcome of investigations concerning immunity.

In 1889, Charrin and Roger noticed that the bacillus pyocyaneus grows in the form of clumps in the undiluted serum of animals rendered immune from this bacillus, whereas in normal serum it grows with diffuse clouding of the medium. This is the first observation of the property of immune serum to cause agglomeration of specific bacteria.

In 1891, Metchnikoff observed the same phenomenon together with immobilization of the bacteria in cultures of the vibrio Metchnikovi, and also clumping of the pneumococcus, in their immune sera and he said: "This fact, presenting a general importance, should be investigated more fully." He did not, however, pursue the investigation, and as he failed to find the same behavior of the hog-cholera bacillus[1] in its immune serum, he seems to have abandoned the idea first expressed as to the general importance of the phenomenon.

In 1893, Issaeff, in the Pasteur Institute, and in 1895, Washbourn, confirmed Metchnikoff's observation as to the pneumococcus, and in 1894 Issaeff and Ivanoff, in Koch's Institute, made the same observation regarding the vibrio of Ivanoff.

In 1894, Pfeiffer, in conjunction with Issaeff, published his important studies on immunity from Asiatic cholera, in which he showed that cholera spirilla introduced into the peritoneal cavity of immunized guinea pigs, or introduced together with immune serum into the peritoneal cavity of normal guinea pigs, quickly lose their motility and break up into small granules. This behavior in the animal body, known as the

[1] As a matter of fact, the hog-cholera bacillus is agglutinated and immobilized by immune hog-cholera serum, as has been shown by Dawson (New York Med. Journal, Feb. 20, 1897). This is an additional proof, if any were needed, of the fact repeatedly insisted upon by the writer and others in this country, that Metchnikoff's so-called hog-cholera bacillus is not the genuine hog-cholera bacillus discovered and described by Theobald Smith (Welch and Clement, Proceedings of the 30th Annual Convention of the U. S. Veter. Med. Assoc. and 1st Veterinary Congress of America, October, 1893).

"Pfeiffer phenomenon," and demonstrated in 1896 by Pfeiffer and Kolle also for typhoid infection, became the subject of investigation by others, which led to the recognition of the importance and general bearings of the agglutinative reaction of specific sera. The Pfeiffer phenomenon is distinct from the agglutinative reaction upon which the method of serum diagnosis to be here considered is based, and Pfeiffer himself can not be credited with a clear recognition of the diagnostic importance of the latter reaction before it was made manifest by the work of others. He, however, directed attention to the diagnostic employment of serum with his reaction, and his discoveries formed the basis for the later work on the agglutinative reaction and, therefore, they occupy an important position in the history of serum diagnosis.

In 1895, Bordet, in studying the conditions of production of the Pfeiffer phenomenon outside of the animal body, noted that if a small quantity of immune serum be added to a suspension in salt solution or bouillon of cholera spirilla these lose their motility and become agglomerated. The significance of Bordet's observation is that he was the first to dilute the serum. His interest, however, was chiefly in the determination of the conditions causing the disintegration of the spirilla by cholera serum, and it can not be said that, at this time, Bordet had a clear perception of the importance and general significance of the agglutinative reaction.

January 3, 1896, Durham presented to the Royal Society a paper giving the results of investigations in Gruber's laboratory in Vienna. This communication embodies the first thorough and systematic study of the agglutinative and immobilizing properties of immune serum outside of the animal body. In this and the rapidly following papers of Gruber and Durham the real importance and general characters of this reaction with immune serum were for the first time made clear. The macroscopic and the microscopic tests, the importance of dilutions, quantitative estimations of agglutinative power, the value of the test for the differentiation of bacterial species and for the determination of a previous attack of cholera or typhoid fever, and many other details were described.

It seems but a small step to determine whether a reaction which had been demonstrated to characterize the serum of animals and human beings which have recovered from an infection may not also be present during the period of infection, but upon this step depended the applicability of the reaction as a method of clinical diagnosis. It was Widal who took it and thereby made available for the diagnosis of an infection a reaction which had previously been thoroughly worked out by Gruber and his collaborators so far as immune serum is concerned.[2] Widal's first communication was presented to the Société Médicale des Hôpitaux on June 26, 1896. This first paper has been followed by numerous important contributions by Widal and others to the same subject.[3]

Nature of the agglutinative property and reaction.—As the result of infection with many bacteria or of intoxication with their products, the blood, even when highly diluted, acquires the property of causing loss of motility and clumping together of the specific bacteria concerned in the infection or intoxication. The clumping is called by Gruber agglutination, and is attributed by him to the presence of substances to which he has given the name agglutinins. He supposes that these agglutinins make the gelatinous capsules of the bacteria swell up and thereby stick the bacteria together. Although there is no proof of this theory, the names "agglutination," to designate the phenomenon, and "agglutinins," for the supposed substances causing it, have been widely adopted.

In the case of motile living bacteria two phenomena characterize the complete reaction, paralysis or immobilization of the bacteria and clumping. Usually these two phenomena go hand in hand, but sometimes there is loss of motility with little clumping or clumping without much cessation of motion, so that the opinion has been expressed that the paralyzing and the agglutinative substances are not identical. The more common deviation from the usual course of the reaction is the occurrence of clumping, with partial preservation of motility.

The agglutinative serum reaction appears to be of wide, although not universal, application, both for motile and non-motile pathogenic bacteria, having been demonstrated for typhoid, Asiatic cholera, pneumococcus infection, tetanus, pyocyaneus disease, glanders, hog-cholera, Malta fever, colon infection, proteus infection, psittacosis, and several other infections.

The change in the blood upon which the reaction depends is doubtless a specific one in the same sense as are the antitoxic, lysogenic, and other specific alterations caused by the action of definite bacteria or their products. It is upon this specificity that the diagnostic value of the reaction is based. It is true that, as in the case of the antitoxic, lysogenic and other protective modifications of the fluids of the body, the normal blood may possess in some degree the same property, so that the specific character of the change may not be apparent without resorting to considerable dilution of the blood or serum. Normal blood may agglutinate, to some degree, not only the typhoid bacillus but various other bacteria. The specificity of this change resulting from infection with a given microörganism is made apparent by increase of the agglutinative power of the serum only for that microörganism, or to some extent also for closely allied microörganisms. The increase of reaction in some degree with closely allied bacteria, does not militate against the specificity of the change, for it is only an expression of the natural affinities between varieties and races of organisms, as between the typhoid bacillus and the bacillus of psittacosis, or between the cholera spirillum and certain other spirilla, or between the varieties of proteus bacilli or of colon bacilli. The development of the specific agglutinative properties of the blood in typhoid fever affords additional proof, if any were needed, that the bacillus typhosus is the specific cause of this disease.

The blood acquires the specific agglutinative power at a variable period, usually within a few days, after the entrance of the pathogenic microörganism or its products. This power tends to increase, but with much irregularity, during the course of the infection, and gradually to diminish and finally to disappear weeks, months, or it may be years after recovery from the infection. Widal lays much emphasis on the reaction being one of infection and not of immunity. Still it is to be noted that by following procedures for raising experimental immunity to great heights, there may be a corresponding rise of agglutinative power, whereby degrees of this power may be attained which are entirely unknown during natural infections. Thus, Widal, by successive inoculations of an ass, has secured typhoid serum with an agglutinative strength of 1 to 43,000, and Salimbeni has obtained, experimentally, cholera serum agglutinating in a dilution of 1 to 50,000. Gruber speaks even of immune sera which agglutinated distinctly in a dilution of 1 to 500,000. There is, however, no necessary correspondence between the height of immunity and that of agglutination; especially may the latter lessen or disappear when the former is preserved. In the light of the experimental results, and for other reasons, it seems to me somewhat misleading to designate the reaction as merely one of the period of infection.

We are not informed as to the nature of the rela-

[2] It appears that Grünebaum, working in Gruber's laboratory, had before Widal's first publication determined the agglutinative property of the blood serum during the period of typhoid infection, but the results of his investigations were not published until after several papers by Widal and others had appeared.

[3] A summary of their own work on serum diagnosis, as well as that of others, with references to literature, is presented in a recent elaborate paper by Widal and Sicard in the Annales de l'Institut Pasteur, 1897, No. 5.

tionship between the agglutinative and the protective properties of the blood. Gruber has based a new theory of immunity on the agglutinative reaction, but this theory is opposed by many facts and can not be accepted. The agglutinative property has been shown to be distinct from the bactericidal, lysogenic, antitoxic and other known protective properties of the blood. At present we have no satisfactory evidence that the agglutinative reaction is concerned in any of the defensive mechanisms of the body. According to Salimbeni, whose results, however, are not in entire accord with those of Durham, agglutination of bacteria does not take place within the animal body, although it has been demonstrated that the living blood plasma possesses the agglutinative property.

We do not know the origin or nature of the so-called specific agglutinating substance, save that in some way it results from the activities of bacteria or their products within the living body. Gruber believes that it is derived from the bodies of the bacteria through the agency of the cells. Bordet considers that it is secreted by leucocytes. Experiments of Widal and Sicard, and of Achard and Bensaude, indicate that it is not secreted by leucocytes outside of the living body, but there is nothing which shows that it may not be formed by cells within the body.

The agglutinative substance is generally believed to be a proteid, as it is precipitated from blood plasma with fibrinogen and globulin, and from milk with lacto-globulin and casin, but it is possible that it is simply mechanically retained by these albuminous precipitates. In typhoid fever it may be absent from albuminous urine and may be present in urine which gives no reaction for albumin (Widal). It behaves like an albuminous substance as regards filtration and dialysis.

The question whether it is strictly proper to speak of agglutinative, bacterolytic, antitoxic, substances in the blood is a legitimate one. They have never been isolated as chemical substances. Behring has expressed the opinion that isolation of antitoxin will never be accomplished, for, in his opinion, it is a force pertaining to highly organized material, and there is no more possibility of separating it as a substance than of isolating the magnetic force from an iron magnet. In all probability these various properties, agglutinative, bacteriolytic, antitoxic, belong to the same general category, and we may look upon the agglutinative property also as a physical one which proteid substance may acquire as the result of the activities of bacteria or their products, and no more separable in the form of a chemical substance than is electricity.

The agglutinative property is quite resistant to injurious agencies. It survives desiccation of the blood or serum for months, and may persist for months in blood serum, even when this is seriously contaminated with microörganisms. It is not destroyed by sunlight, unless overheated. It is weakened by prolonged heating at 60 degrees C., and is annulled by heating for ten minutes at 75 to 80 degrees C.

The agglutinative property in typhoid fever is present in maximum amount in the blood, being somewhat greater in the blood plasma than in the blood serum. It is found in blister serum in essentially the same strength as in blood serum. In other fluids, as the pleural, peritoneal, pericardial, inflammatory and edematous, it is in smaller and variable amount. In milk and colostrum it is present in marked degree. It is weak and inconstant in the urine, bile and aqueous humor. It has been found in tears naturally secreted, but is said to be absent from those provoked by irritants. It may be present in typhoid stools (Block). It has not been found in the cerebrospinal fluid or the fluid in the seminal vesicles. It has not been positively determined whether the distribution of the agglutinative property in the various humors of the body outside of the blood, can be explained wholly by processes of filtration and diffusion from the blood plasma, although these are doubtless the main factors.

The agglutinative reaction may or may not be obtained with the blood of a fetus or new-born infant of a mother with typhoid fever. Chambrelent and Saint-Philippe consider that the presence or absence of the reaction in the fetus depends on whether or not the typhoid bacilli break through the placental barrier from mother to fetus and cause infection of the latter. Further investigations are needed to determine this point and thus to decide whether the agglutinative property is passively or actively acquired by the fetus. For it has been shown that this property, like immunity, not only may be actively acquired as the result of infection or intoxication, but may be passively transmitted by injection of agglutinative serum, the reaction in the latter case appearing promptly without symptoms of infection. being relatively slight, and disappearing after a short time.

Courmont thinks that the development of typhoid bacilli in a fluid robs it of agglutinative power. Thus he has found that the vegetation of the bacilli in typhoid serum deprives it, in a few days, of the agglutinative property, and that blood obtained, post-mortem, from the spleen, liver and mesenteric glands, organs in which the typhoid bacilli are especially abundant, is poorer in agglutinative power than that from other parts. Ménétrier found in a case of typhoid fever that the pleural exudate, which usually gives the agglutinative reaction, did not do so, and that it contained typhoid bacilli in large number. It has been suggested that these observations may shed light on the exceptional cases of typhoid fever with absence of the specific serum reaction. Flexner has shown that, occasionally, the typhoid bacilli develop in the blood in such large numbers as to produce a genuine typhoid septicemia. Still these observations, interesting as they are and deserving further investigation, must be interpreted with caution, for Widal has found a purulent exudate from an immunized ass, swarming with typhoid bacilli, to present, even after fifteen months' preservation, an agglutinative power of 1 to 13,000, the power of the blood serum of the same animal kept for the same length of time being 1 to 14,000.

We have no satisfactory explanation of the production of the phenomenon of agglutination by specific serum. Gruber's explanation already mentioned has received no confirmation. The phenomenon occurs with non-motile as well as with motile bacteria, with dead as well as with living organisms. Typhoid bacilli killed by formol in weak solution, or by heating for five minutes at a temperature of 56 degrees C., are about as sensitive to the reaction as are living bacteria, and retain their agglutinability for a long period (Widal). The phenomenon, therefore, is a physical rather than a vital one, although probably dependent in some way on the protoplasmic constitution of the bacterial cell. Salimbeni, as has been stated, found that the phenomenon does not occur in the living body of immunized animals. He also found that the presence of atmospheric air greatly

favors the reaction, it requiring much more concentrated serum and a longer time to bring about the phenomenon in a vacuum than when the fluid is exposed to the air. Widal has shown that other physical conditions, particularly contact with objects, such as the surface of slide or cover-glass, and partial evaporation, favor the production of the reaction.

Agglutinative serum is often likewise bactericidal and bacteriolytic, but the agglutinative reaction is independent of the bactericidal and is manifest with dilutions of the serum which annul the bactericidal power. In such dilutions the agglutinated and immobilized bacteria are not altered morphologically, or in staining properties, or in pathogenic power, or in any other way, so far as has been determined. Whether the temporary inhibition of bacterial growth in agglutinative serum is dependent on the agglutinative or on some other property of the serum is not known.

Methods of making the serum test for typhoid fever.—Widal, in his first communication, described both the slow or macroscopic and the quick or microscopic methods. For each of these he recommended a dilution of one part of blood or blood serum to ten parts of the fluid containing the culture. The macroscopic method consists in adding the blood or serum to be tested either to a young bouillon culture of the typhoid bacillus or to sterile bouillon which is then at once inoculated with the bacillus. In the former case the reaction with typhoid serum appears usually within two or three hours and consists in clarification of the previously turbid fluid, and the formation of a clumpy sediment composed of accumulated bacilli. In the latter case the tube is placed in the incubator and within fifteen hours the reaction is manifested by growth of the bacilli in the form of a sediment at the bottom of the tube, the fluid remaining nearly or quite clear.

The microscopic test, to which Widal gave the preference, is made by mixing the blood or serum with a young bouillon culture or with a suspension in bouillon or salt solution of a fresh growth of the typhoid bacillus and examining a drop or two of the mixture at once under the microscope. With a dilution of 1 to 10 this microscopic typhoid reaction appears, as a rule, immediately or within a few minutes, and is evidenced by loss of motility and by clumping of the bacilli into masses of various sizes and shapes.

Widal obtained the blood either with a sterilized hypodermic syringe from a vein of the arm, or by pricking the finger. It may also be conveniently obtained by pricking the lobule of the ear. A few drops of blood suffice for collecting the necessary amount of serum; indeed a single drop will do for the reaction. The blood may be collected in a small test tube where, usually in a few minutes, it clots. The separation of the serum may be facilitated by passing a sterilized platinum needle between the glass and the clot, or by the centrifuge, or the blood may be collected and allowed to clot in a slanted tube, which is then placed upright, the separated serum trickling to the bottom.

In a communication made July 31, 1896, Widal said that results equaling those with blood serum can be obtained with blister serum, and this procedure has been employed with much satisfaction by the Health Department of New York city.

At the same time Widal called attention to the preservation of the agglutinative property in dried blood and serum. Wyatt Johnston deserves the credit of developing the test with dried blood and for introducing the method of serum diagnosis into the work of municipal laboratories. The dried-blood method, which has been used far more extensively in Canada and this country than in Europe, possesses certain manifest advantages, especially ease of collection, freedom from subsequent contamination and readiness of transportation, and it has given excellent results in the hands of Johnston and others. The principal objection, and this is of considerable importance when precise results are desired, is the difficulty of obtaining accurate quantitative dilutions with the use of dried blood.

Several observers, including Breuer, Haedke, Du Mesnil de Rochemont, Scheffer, have expressed the opinion that the macroscopic method is more trustworthy than the microscopic. This I believe to be an error and to be due to unfamiliarity with all of the conditions essential for the accurate employment of the microscopic test. The latter is more delicate, prompt and precise than the macroscopic reaction, and requires less care in respect to accidental contamination.

A year's experience with the method of serum diagnosis of typhoid fever has led to a general consensus of opinion as to its great value. It has, however, been recognized that certain precautions in the application of the test are necessary in order to avoid mistakes. Numerous modifications of the original methods have been suggested, the most important relating to quantitative determinations. In considering the value and practical utility of such modifications of the test, several points should be borne in mind. Practically all of the methods recommended by competent investigators have given good results in the great majority of cases. For clinical purposes it is desirable that neither the method of obtaining and collecting the blood nor that of conducting the test should be made more difficult and complicated than is absolutely necessary. Methods which may be essential for exact scientific work, where every possible source of fallacy is to be avoided, may not be the best for the routine examinations of a clinical or a municipal laboratory. Where absolute accuracy is not obtainable it is upon the whole better that the method should err on the side of now and then including a non-typhoid case than in excluding cases of genuine typhoid fever. With due allowance for such considerations as these, we must welcome all efforts to give greater precision to the methods of serum diagnosis and to determine the capabilities of these methods and their possible sources of error. In exact quantitative work with the serum test the most important points to be considered are the characters of the culture, the dilution of the serum, the time limits, the criteria of the reaction and certain physical conditions influencing the reaction.

Characters of the culture.—There has been considerable difference of opinion as to whether cultures of the typhoid bacillus obtained from different sources are equally sensitive to the agglutinative reaction. Widal, Durham, Stern and C. Fraenkel, who have all had large experience with different cultures, have found only unimportant and inconstant differences in susceptibility to the reaction. The fact determined by Pfeiffer that the less virulent the culture, the greater the sensitiveness to the lysogenic reaction (Pfeiffer's phenomenon), seems to have been considered by

many without sufficient investigation to be equally applicable to the agglutinative reaction. Kolle, without however presenting sufficient evidence, emphasizes the greater susceptibility of cultures with weakened virulence to agglutination. The most satisfactory evidence on this point is furnished by Kühnau, who made a careful comparative study of the behavior with the serum test of a non-virulent and a virulent typhoid culture, and found the former to react much more intensely with normal and typhoid sera. He, therefore, lays stress on consideration of the virulence of the culture in quantitative work with the serum test. In view of the conflict of opinion further investigations upon this question are needed.

It cannot be doubted that several observers have had to do with typhoid cultures which presented distinct differences in susceptibility to the agglutinative reaction. Especially worthy of consideration, although not wholly in accordance with some results of others, are the observations of Johnston and McTaggart, confirmed by Appel and Thornbury, that solutions of dried blood are more potent than serum in agglutinative power, although not in paralytic effect, and that such solutions from non-typhoid cases are prone to give partial (pseudo-) reactions with frequently transplanted typhoid cultures, whereas this difficulty is largely overcome by using fresh cultures planted from stock cultures a month old. Hence they strongly recommend for the dried-blood method cultures of the latter character. They, as well as other writers, likewise emphasize the importance of considering the composition of the culture medium, which should be favorable to vigorous growth and not too strongly alkaline.

Only young cultures should be used, preferably not over twelve to eighteen hours old, if grown in the incubator. Older room cultures can be used. Old cultures agglutinate more readily than young ones. Either bouillon cultures or suspensions in bouillon from solid cultures may be employed. There is no difficulty in securing uniform suspensions of isolated, actively motile typhoid bacilli, especially from young cultures on dried-out agar. In every case it is of prime importance to make a control examination of a drop from the same part of the culture or suspension which is used for the test and at the time of making the test in order to be sure that there are no preëxisting clumps, that the bacilli are actively motile, and that the culture is not contaminated.

Stern suggested that the concentration of the suspension, that is the number of bacilli in it, may be a factor meriting consideration, and Kühnau and Block have shown that this is the case. Weak suspensions are more readily agglutinated and paralyzed than stronger ones. Hence, Kühnau recommends the use of suspensions of known concentration, which can be approximately secured without much difficulty. He uses a suspension in bouillon of a fifteen-hour virulent agar culture (grown in the incubator) containing about one hundred and twenty million bacteria in a cubic centimeter.

Dilution of the serum.—Inasmuch as normal and non-typhoid blood may possess distinct agglutinative property, especial importance is attached to dilution of the serum, in order to avoid mistaking the normal reaction for one of typhoid fever. The opinion has been widely expressed that the dilution recommended by Widal, 1 to 10, is too low, and that a dilution should be used which is not known ever to give a reaction with non-typhoid blood. The fixation of the upper limit of such a dilution has been placed gradually higher and higher, thus by du Mesnil at 1 to 25, by Kolle 1 to 30, by Grünbaum 1 to 33, by Stern 1 to 40, by Kühnau 1 to 50. Even if it should be admitted that a reaction in non-typhoid cases with these higher dilutions is ever of such a character as might mislead an experienced observer, its occurrence is, according to most observers, very exceptional.

The question arises whether the adoption of a dilution of say 1 to 50 as the standard, would result in the exclusion of genuine typhoid cases from the diagnosis. Widal divides the typhoid cases in which he has measured the agglutinative power of the blood into five groups: *a*, those with very weak power, less than 1 to 100 (four cases); *b*, with weak power, between 1 to 100 and 1 to 200 (nine cases); *c*, with medium or average power, from 1 to 200 to 1 to 500 (eight cases); *d*, with high power, from 1 to 500 to 1 to 2,000 (nine cases); and *e*, with very intense power, exceeding 1 to 5,000 (three cases). In only one case did the agglutinative power not rise over 1 to 40, it being 1 to 30 on the twentieth, and 1 to 40 on the twenty-second day of the disease. In one case, Widal found the strength to be 1 to 12,000. In nineteen cases measured by Stern, the agglutinating strength was never less than 1 to 50. C. Fraenkel found the average to lie between 1 to 100 and 1 to 200, sometimes reaching 1 to 5,000. Out of seven cases Kühnau found two in which the serum was active only in dilutions less than 1 to 50, it being 1 to 30 in one, and 1 to 20 in the other case, but in both he made a positive diagnosis of typhoid fever in consequence of disparity of the action of the serum on the colon bacillus and the typhoid bacillus. From the observations thus far reported, although they are insufficient in number for definite conclusions, there would seem to be only small liability of failure to recognize genuine typhoid cases by resorting to dilutions of 1 to 40 or 1 to 50, but unquestionably a few cases would escape recognition, and for this reason lower dilutions should also be used, and if those between 1 to 10 and 1 to 50 give decided reaction there should be, at least, suspicion of typhoid fever.

It is not, therefore, to be recommended that one should make the test with only high dilution, such as 1 to 50. The negative result of a preliminary test with equal parts serum and culture suffices to exclude typhoid reaction. The examination, if positive, may then be made with a low dilution of the serum and for this Widal's recommendation of 1 to 10 or 1 to 15 may be well adopted. If with this dilution the microscopic reaction is complete and almost immediate, as is often the case, there is practically no risk in making a positive diagnosis. But for absolute certainty and above all in cases where the result of the reaction is not prompt, complete and unmistakable, higher dilutions should be employed; if the amount of serum permits only one such, it may be 1 to 50, but preferably intermediate dilutions should also be made, and it is desirable, if not absolutely necessary, to try dilutions higher than 1 to 50. For making the dilutions there are various simple technical procedures, which involve but little expenditure of time and labor and only small quantities of serum, as, for example, that recommended by C. Fraenkel. An accurate fixation of the upper limit of agglutinative power is often tedious and not generally necessary in diagnostic work. A positive diagnosis of typhoid fever, based exclu-

sively on the test with a low dilution, in a case which subsequently proves not to be typhoid should not be considered as in any way invalidating the results of an accurate employment of the method of serum diagnosis.

It is self-evident that the employment of varying degrees of dilution of the serum, without at the same time taking into consideration other factors which influence the reaction, has little sense and does not constitute, in itself alone, an accurate method of mensuration of agglutinative power.

Time limits.—As the rapidity with which the reaction appears and progresses generally varies, other things being equal, according to the agglutinative strength of the blood it is evident that methods of exact mensuration of this strength must take into consideration the length of time required for the development of the reaction after the addition of the serum. Many writers have not paid much attention to this point. Stern has proposed that a limit of two hours be adopted as an arbitrary standard for the microscopic reaction, and Widal has accepted this proposal. With this unit, an agglutinative power fixed at 1 to 500 means that 1 part of serum added to 500 parts of the fluid containing the culture, agglutinates and paralyzes the bacilli within two hours, although a higher dilution may give a decided reaction in six or eight hours. The optimum effect is, according to Stern, not attained before the lapse of six or eight hours.

By varying the time limits, results obtained by lower dilutions may be roughly comparable with those by higher dilutions. Thus, for diagnostic purposes, a fifteen minute time limit for dilutions of 1 to 10 may be adopted and a two hour time limit for dilutions of 1 to 50 or higher, but it should be understood that in all doubtful cases quantitative determinations by varying the dilution should be employed.

Criteria of the reaction.—Some writers have proposed to make either the cessation of motility or the clumping the essential criterion of the reaction. Thus Stern selects the clumping and Kühnau the paralysis of motion. In my judgment both phenomena enter equally into the reaction and deserve equal consideration, so that a reaction is not to be considered complete and satisfactory unless the bacilli are both clumped and rendered immobile. Partial reactions in which one or the other characteristic is lacking may warrant suspicions and lead to further examination, but they should not be made the basis of positive diagnosis. For this reason the use of killed cultures, as suggested by Widal, while it may have a limited field of application, can not supplant the ordinary method. As already stated, the microscopic reaction is to be preferred to either of the macroscopic methods, although the latter afford striking objects for demonstration. With low dilutions bactericidal and lysogenic phenomena are common, but they do not pertain to the agglutinative reaction itself.

Certain physical conditions influencing the reaction.—As already mentioned Salimbeni has shown that free exposure to the air favors the reaction and Widal has pointed out that partial evaporation at the edge of the cover-glass and contact of the specimen with slide and cover-glass are also favoring conditions. Hence, the conditions for the reaction are not exactly the same with the serum bouillon mixture in thin layer beneath the cover-glass on an ordinary slide, as in a thick layer, or in a sealed drop culture on a hollow slide, or in a column of fluid in a test-tube, or in a moist chamber. The presence of fibrinous masses, granules and material foreign to blood serum may perhaps explain in part the greater frequency of partial reactions with normal blood when the dried blood method is used than when the serum method is employed. The temperature of the incubator, by favoring evaporation and in other ways, accelerates the reaction. For exact quantitative work these various physical conditions need consideration and further investigation. Widal prefers the use of ordinary slides to that of hollow-ground slides, and does not advise keeping the specimen in the incubator. It is not to be supposed that the diagnostic use of the serum test generally hinges on such delicate points as these, but they are among the points to be considered in the explanation of certain irregularities in the results of the test, in comparing the results of different workers, and in mensuration of agglutinative power, especially with high dilutions.

Reactions with the colon bacillus.—Statements of different writers as to the occurrence of the agglutination of the colon bacillus with normal and typhoid sera are not harmonious. Widal and Courmont find that all human sera, whether normal or typhoid, have a slight agglutinating action on the colon bacillus in dilution of 1 to 10, whereas normal sera have only exceptionally any such action on the typhoid bacillus in this dilution. Many observers have noted some agglutination of colon bacilli with typhoid serum, although the reaction is much less intense than with the typhoid bacillus. Vedel found, in a case with symptoms of typhoid fever but without the typhoid serum reaction, marked colon reaction, and he interpreted the case as one of colon infection simulating typhoid. He is not, however, inclined to attach much diagnostic importance to the colon reaction, as he found that it might be well marked both with normal and typhoid blood. Johnston and McTaggart found genuine colon reactions with typhoid blood to be rare, provided the typhoid reaction was well marked. In several cases, however, where the symptoms suggested typhoid but the typhoid serum reaction was absent, they found marked colon reaction. They are inclined, therefore, to attach diagnostic importance to the latter reaction. The colon cases were mild and of shorter duration than ordinary typhoid fever.

Kühnau makes use of the colon reaction to assist in the diagnosis of typhoid fever in doubtful cases. He finds that normal serum reacts in the same way with both colon and typhoid bacilli, whereas typhoid serum, even when of weak specific power, reacts much more intensely with the typhoid bacillus than with the colon bacillus. By availing himself of this unequal action of typhoid serum upon the two species of bacteria, he felt justified in making the diagnosis of typhoid fever when the agglutinative power of the serum did not exceed 1 to 20. Kühnau's suggestion is interesting, but further investigations are needed to determine its value.

We must also await further studies before Johnston and McTaggart's highly suggestive views as to the existence of colon infections simulating typhoid and capable of diagnosis by the serum reaction with the colon bacillus can be accepted.

As was first pointed out by me in 1890, the colon bacillus is an extremely common secondary invader of the body in all sorts of conditions, particularly those

with lesions of the intestine. It can very frequently be found in internal organs outside of the intestine in typhoid fever, if careful search is made. We have no satisfactory proof that it produces either symptoms or lesions in most of these cases, and one would expect more common and intense serum reactions with bacillus coli in typhoid fever, if the organism was engaged in pathogenic work. The writer has repeatedly taken occasion to protest against what seem to him unwarranted inferences as to the pathogenic significance of the mere detection of the colon bacillus in the internal organs at autopsies, although there can be no question that under certain conditions this bacillus may be pathogenic for man.

As the colon group of bacilli contains numerous races, some approaching the typhoid bacillus closely, it is to be expected that they will vary markedly in their sensitiveness to agglutination with different sera.

Durham found that typhoid immune serum in no instance produced any agglutinative reaction with ten different specimens of the bacillus coli obtained from various sources. Colon immune serum reacted on its own race of bacilli exactly like typhoid serum on typhoid bacilli, but it did not react with all races of colon bacilli, a graduated series of effects being observed with different specimens of these bacilli. Rodet, however, whose results are reported in much less detail than those of Durham, found a certain degree of reciprocal action between colon and typhoid immune sera and their respective bacteria.

Date of appearance and disappearance of the typhoid serum reaction. Absence of reaction.—The presence of the specific agglutinative reaction can usually be counted on by the end of the first or the beginning of the second week of typhoid fever. It may appear as early as the second day of the disease (Johnston and McTaggart, C. Fraenkel), but this is very exceptional. It may here be noted that the determination of the exact day of a disease, often so gradual and insidious in its development as typhoid fever, must frequently be more or less arbitrary, and will vary according to the case and with different observers. Sometimes the first appearance of the reaction is delayed, exceptionally until the end of the second or into the third week, or even later. There are rare cases in which the reaction is missed during the first attack and makes its appearance in the relapse (Breuer, Thoinot, Biggs and Park, and others). It has even been missed until the first days of convalescence. Blumenthal relates an interesting case in which the reaction was absent during the fever, tests being made on the twelfth and twenty-first days with serum dilutions of 1 to 10, but it was found with dilutions of 1 to 100 two days after the beginning of apyrexia. Achard likewise once found the reaction only during convalescence. We have not at present a sufficient number of accurate data to furnish definite figures as to the frequency of these delayed reactions, but their occurrence undoubtedly constitutes a defect in the method of serum diagnosis of some importance. A negative result of the test does not exclude the diagnosis of typhoid fever. The probability against this diagnosis is the greater, the later the period of the fever in which the negative result is obtained and the oftener the examinations are repeated. As regards the interpretation of negative reactions, the serum test does not differ from other bacteriologic diagnostic tests, that for the tubercle bacillus for instance.

There are authentic cases of typhoid fever in which repeated examinations of the blood during the course of the disease and its convalescence failed to reveal the specific agglutinative reaction, even with serum dilutions of 1 to 10. We can not at present say what percentage of the total number they make. Widal and Sicard found absence of the reaction in only one out of 163 cases of typhoid fever examined by them. In this negative case, in which the diagnosis was confirmed by cultivation of typhoid bacilli obtained by hypodermic puncture of the spleen, the reaction was absent during the fever, the apyrexia, the relapse and the convalescence. Of 116 cases of typhoid fever examined by Courmont, the reaction appeared in all, being delayed after the eighth day in only five. Of 70 cases examined by Chantemesse, it was present in all. Of 129 cases examined by Johnston and McTaggart, if a few cases examined only late in convalescence, or at a very early stage without reëxamination, be excluded, the reaction was missed in only one. In many reports cases, believed to be typhoid, are recorded as giving negative reaction when only one examination was made, this being sometimes early in the disease. Such cases doubtless belong mainly to the group with delayed reaction. The importance of repeated examinations is illustrated by such observations as Stern's, in which the test was negative at the end of the second week and positive two days later; of Widal's, negative on the tenth, positive on the twenty-second day, and several others of similar purport.

The agglutinative power of the blood tends to increase during the progress of the fever, but there are exceptions, and in general the intensity of the reaction is subject to irregularities and oscillations, which may be notable from day to day. There may be marked sudden rise or fall of reactive power. While weak reactions are more common in mild cases, there is no definite correlation between premature or delayed development or the intensity of the reaction and the gravity of the disease. The persistence of high agglutinative power, for example 1 to 2,000, after subsidence of the fever, does not prevent relapses.

In the majority of cases the specific agglutinative power of the blood diminishes in the first weeks or months after cessation of the fever and disappears within a year. Exceptionally it may vanish as early as eight or ten days after the fever. Widal and Sicard noted its disappearance on the eighteenth and twenty-fourth days, Breuer on the seventeenth and twenty-fifth days, E. Fraenkel on the twenty-eighth day after defervescence, etc. Disappearances at such early dates as these are, however, not the rule. According to Courmont's experience, the serum reaction disappears in children most frequently during the course of the first two months, and in adults toward the fifth and sixth months, although it is not uncommon for it to continue a year. The specific reaction may, however, persist for years, perhaps indefinitely. Of forty cases which had had typhoid fever at least a year before examination, Widal and Sicard found the agglutinative reaction, either marked or slight, in eleven; after one and one-half year, one case, reaction weak; after two years, one case, reaction weak; after three years, two cases, in one marked, in the other weak; after six years, one case, reaction 1 to 10; after seven years, one case, reaction marked; after eight years, one case, reaction 1 to 1,800; after nine years, three cases, one marked, one 1 to 40, one 1 to 30; after twenty-six years, one case, 1 to 30. Kühnau found

after one year, two cases with reaction of 1 to 80; after two years, one case, reaction 1 to 60; after seven years, one case, reaction 1 to 60. All others of a series examined (total number not stated) showed negative reaction after one year. It was observed by Widal and Sicard that in contrast to the reactions during infection and for the first weeks after defervescence, those of long standing showed no notable fluctuations in intensity during the periods of examination, extending sometimes over several weeks.

The persistence of the specific reaction after typhoid fever is of importance from two points of view, retrospective diagnosis and interpretation of the diagnostic significance of the reaction during a febrile infection. It is sometimes of interest and practical importance to determine that an individual has previously had typhoid fever. Thus Courmont was able by the serum diagnosis to determine that a patient with multiple neuritis, supposed to follow an attack of dysentery, was in reality convalescent for a month and a half from typhoid fever, and Achard recognized the real nature of an attack of osteomyelitis in a patient who had had typhoid fever a year before (cited from Widal and Sicard)

Many writers have called attention to the evident possibility of a mistake in diagnosis when the serum reaction is found in a person with a febrile infection, who has recovered from typhoid fever, although it does not appear that any serious difficulty has been encountered thus far from this source of error. It, however, indicates the importance of obtaining a careful history of the patient, not only as regards recognized typhoid fever, but as to attacks interpreted as dysentery, gastric fever, appendicitis, malaria, etc. An observation reported by Stern indicates that the specific typhoid reaction may be acquired even without manifest illness. He suggests that careful quantitative estimations of agglutinative power may restrict the possibility of error in diagnosis arising from long persistent reactions, as increase or diminution in the course of the fever or of the convalescence would speak for fresh infection. Even if the fullest possible allowance be made for this source of error, it applies to so small a number of cases that the value of the method is not seriously impaired.

Presence of the reaction in non-typhoid cases.—The blood of many hundred persons, either healthy or affected with diseases other than typhoid, has been tested for the specific typhoid reaction, and it can now be asserted that a serum reaction which an experienced observer using accurate methods would consider characteristic of typhoid fever, is to be found only most exceptionally in those who have not had typhoid infection.

Several observers, especially the Germans, find that by adherence to Widal's original directions mistakes may occur, but that these can be avoided by attention to quantitative determinations, especially dilution of the serum, time limits, and characters of the culture used for the test. The most remarkable observations on this point come from Breslau and are reported by Stern and Kühnau. Stern examined the blood serum of seventy persons not suffering from typhoid fever and, according to their statements, never having had typhoid fever. In twenty of these the serum had an agglutinative strength of 1 to 10, in five a strength of 1 to 20, and in two a trace of reaction was obtained with dilutions of 1 to 30. In none of these cases did he find any reaction with dilutions of 1 to 40. Of more than fifty similar cases examined by Kühnau, in forty-one no reaction was obtained with dilutions higher than 1 to 5. In eight agglutination was observed with dilutions of 1 to 10, up to 1 to 20; in four with 1 to 30; in three with 1 to 35, up to 1 to 40; and in one even with 1 to 50.

In judging these results, apparently so divergent from those of Widal and nearly all others, it is to be noted that both Stern and Kühnau used the microscopic reaction, made two hours the time limit, even for the lowest dilutions, kept the specimens in the thermostat, and that Stern regarded the clumping and Kühnau the paralysis of motion as the criterion of the reaction, thus apparently recording as genuine what many others would consider partial or pseudo-reactions. It is clear that those who interpret only complete reactions occurring within fifteen to thirty minutes at room temperature as genuine reactions, would not be likely to obtain any such results as those reported by Stern and Kühnau. Nevertheless, it must be admitted that even partial and late reactions are unwelcome and disturbing, even if an observer thinks that his skill and experience will enable him to avoid mistakes from their occurrence. It may also well be, as suggested by C. Fraenkel, that Stern's and Kühnau's normal and non-typhoid cases include not a few who had recovered from unrecognized typhoid infection, and this supposition is the more probable in consequence of the prevalence of typhoid fever in Breslau. Stern himself calls attention to the liability of failure to recognize certain mild cases.

There have been a few cases reported in which the diagnosis of typhoid fever was made on the basis of the serum reaction, but which the authors, from subsequent developments, considered to be free from typhoid infection. It has been claimed, therefore, that positive serum reactions are not a sure sign of typhoid fever, although no one claims that the chances of error from this source are more than slight. The cases reported by Achard and Bensaude, Jez, Ferrand, du Mesnil and van Ordt have given rise to the most discussion. For what seem to me justifiable criticisms of these reports, I would refer especially to the papers of Widal and of Stern, and I will limit my remarks to some general statements concerning such alleged failures of the serum test.

In the first place, as the matter now stands, positive reactions obtainable only with dilutions lower than 1 to 50, possibly than 1 to 60, especially if the reaction is partial and late in appearance, are not certain diagnostic signs of typhoid fever. In most of the cases just referred to no exact quantitative estimations of the agglutinative strength of the serum were made, and hitherto it has not been shown that the reaction ever occurs with non-typhoid serum in dilutions exceeding 1 to 50, with observance of other quantitative points which I have already considered. With the limited number of observations, however, which we now possess, we can not, of course, say but that such cases will be found in the future.

In the second place, infection with the typhoid bacillus can not be positively excluded either on clinical grounds alone or by anatomic examinations at the postmortem table. Infections with the typhoid bacillus occur without any characteristic anatomic lesions. There may be entire absence of ulcers or other lesions of the intestine. We have recently had at the Johns Hopkins Hospital a case with positive serum reaction, from which Dr. Flexner cultivated, in large num-

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ber, typical typhoid bacilli from the gall bladder although there was no previous history of typhoid fever, and there were no intestinal lesions. In 1891 I called attention to the favorable conditions offered by the bile for the prolonged survival of the typhoid bacillus. In one case I was able to demonstrate large numbers of typhoid bacilli in the bile of a rabbit, 128 days after intravenous injection of 0.5 c.c. of a bouillon culture[1]. Pick has reported a case with marked positive serum reaction in which at the autopsy no typhoid intestinal lesions and no swelling of the spleen were found, but bacteriologic examination showed the presence of typhoid bacilli, not however in the spleen. Guinon and Meunier's case is instructive. During life the symptoms were those of acute miliary tuberculosis and typhoid fever combined. Serum reaction was positive. At autopsy the lesions appeared to be only those of acute miliary tuberculosis, small ulcers in the intestine being typically tubercular in aspect. Typhoid bacilli, however, were cultivated from the spleen and other parts. As both the symptoms and the bacteriologic examination indicated that the typhoid infection was in course of disappearance, the case, if examined at a somewhat later period, might readily, as Guinon and Meunier remark, be placed to the discredit of the positive value of the serum test. We are justified, in the light of such cases as these, in demanding that thorough bacteriologic examinations be made before cases which have given during life the characteristic serum reaction, but which do not present at autopsy the anatomic lesions of typhoid fever, be recorded as free from infection with the typhoid bacillus.

In the third place, the difficulty of excluding a previous attack of typhoid fever, after which, as already stated, the specific serum reaction may persist for years, is to be borne in mind.

In conclusion, I would emphasize the following practical points:

1. Experience has demonstrated that the method of serum diagnosis of typhoid fever is of great practical value.

2. The alteration of the blood on which this method is based, is a specific effect of infection or intoxication with the typhoid bacillus.

3. The microscopic serum test is to be preferred to the macroscopic methods.

4. Quantitative determinations, relating especially to the culture, the time limits, and the dilution of the serum, are of importance and, at least in doubtful cases, should not be neglected.

5. As the reaction may be delayed or occasionally absent, a negative result of the test does not exclude the diagnosis of typhoid fever. The later in the course of the disease the test is applied, and the oftener the examinations are repeated at intervals, the less is the probability of the existence of typhoid fever.

6. The persistence of the reaction, sometimes for years, after recovery from typhoid fever, is to be borne in mind in interpreting the reaction in febrile conditions. The appearance of the reaction and its increase during the period of observation speak for fresh typhoid infection.

7. The danger of mistakes from positive reactions in non-typhoid cases can be guarded against in nearly all cases.

[1] Welch: Bulletin of the Johns Hopkins Hospital, August, 1891.

8. Provision should be made, especially by the establishment and support of municipal or State laboratories, to render generally available to practitioners the serum method of diagnosis, as well as other bacteriologic procedures of similar practical value.

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A CLINICAL STUDY OF WIDAL'S SERUM DIAGNOSIS OF TYPHOID FEVER.

Presented to the Section on Practice of Medicine, at the Forty-eighth Annual Meeting of the American Medical Association, held at Philadelphia, Pa., June 1–4, 1897.

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The method employed was as follows: A drop of blood was taken from the previously cleaned finger of the patient suspected to be suffering from typhoid fever. This drop of blood was allowed to dry on a cover-glass or microscopic slide and was then taken to the laboratory.

The dried blood was dissolved in one drop of distilled water, at the time of making the test. One platinum loopful of this solution was added to nine platinum loopfuls of a pure bouillon culture of the typhoid bacillus, which was not more than twenty-four hours old. This mixture was then studied in the hanging drop.

These drops were examined from time to time until it was definitely established that the reaction was present or absent. In case the motility of the organisms was still present in the evening, when the light failed, the preparations were put aside in a warm room until morning when they were again examined. If motility was still present and there was no formation of clumps the reaction was definitely considered to be absent.

Early in the course of the examinations, instead of using a solution of blood which had been previously

dried, the drop of blood was allowed to coagulate on the slide and then the serum was drained to one side and the bouillon culture added. This technique produced several faulty results because the undiluted blood serum was liable to cause agglutination on account of the normal agglutines contained in the blood.

At first the bouillon cultures were grown over night in the incubator, at a temperature of 37 degrees. Later the inoculation into bouillon was made in the afternoon and the tube was carried in the vest pocket for several hours and then left in a warm room over night. This method gave a bouillon culture which was particularly suitable for the performance of the test and from which much better results were obtained than from cultures grown by the former process.

In this manner one hundred tests have been made. The blood was taken, in nine cases, from children under 15 years of age and in ninety-one cases it was taken from adults. Twenty-six of the patients from whom blood was taken were females, and seventy-four were males. The age or sex of the patient seemed to make no difference in the reaction. The results of the examinations will be classified under the following heads: 1. Cases of typhoid fever in the course of the disease. 2. Cases of convalescent typhoid fever. 3. Cases in which typhoid fever had been present at some previous time 4. Cases in which typhoid fever was absent.

1. Thirty-five examinations were made of typhoid fever blood, the patient being in the disease and the temperature ranging from 99.6 to 104.6. The reaction was present in all except four cases.

In one instance (obs. 25) the reaction was imperfect, there being a tendency to the formation of clumps of bacilli, but the clumping being irregular and numerous motile bacilli remaining distinct between the groups. This observation was made on the fourteenth day of the disease, at a time when the temperature was 102. Later on in the disease, on the twenty-third day, when the temperature was 102.2 the reaction was present and perfectly developed. In three cases the reaction was absent (obs. 32, 33 and 35). In one of these the examination was made on the third (obs. 35), in another on the fifth (obs. 32), and in the third on the seventh day (obs. 33).

The earliest observation made was on the third day of the disease (obs. 35), and the reaction was then absent. One examination was made on the fifth day and the reaction was absent (obs. 32). Two examinations were made on the seventh day. In one of these cases the reaction was present (obs. 4), and in the other it was absent (obs. 33). The reaction was apparently not affected by cold bathing. Concerning this point, however, the observations have been too few and not sufficiently accurate.

2. Twenty-four observations were made on the blood of patients convalescent from typhoid fever. In these cases the temperature ranged from 99.6 to 97, with two exceptions. In one of the excepted cases the temperature was 103, and was due to an attack of tonsillitis; in the other the temperature was 102.8, and was caused by an attack of post-typhoid meningitis, of which the patient finally died. At the autopsy in this case, healing typhoid ulcers were discovered. Of these observations, the reaction was present in eighteen cases, imperfect in four cases, and absent in three cases. The cases in which the reaction was still present were examined from the twenty-fourth to the sixty-seventh day after the commencement of the disease. In the cases in which the reaction was imperfect, thirty-four, one hundred and four, fifty-eight and one hundred and five days had elapsed from the beginning of the disease. Thirty-eight, eighty-seven and sixty-two days respectively had passed since the beginning of the disease, in the cases in which the reaction was absent.

3. Seven examinations were made of blood from patients in whom typhoid fever had been present at some previous time. A patient who was being treated for chlorosis presented a distinct reaction, and on inquiry it was found that she had suffered from typhoid fever ten years previously.

On a second patient who had suffered from typhoid fever in September, 1896, three examinations were made. Once the reaction was present, once imperfect and once absent.

In the cases of two nurses, who had suffered from typhoid fever in 1893, the blood showed the reaction in one, while the reaction failed to appear in the other. The nurse whose blood showed the reaction had had a more severe attack of the disease.

In the case of a man who had suffered from typhoid fever in January, 1897, the reaction was absent.

4. Thirty-five examinations of blood from patients suffering from other disease than typhoid fever were made. The reaction was absent in all these cases. The diseases from which the patients were suffering were varied. Two cases only of tuberculous disease were obtainable. The blood of these gave a negative result when the test was made. Examinations with negative result were made in two cases of catarrhal fever. A varying list of diseases of the alimentary tract were examined with like negative result. It is a matter of regret that the blood from a case of acute miliary tuberculosis could not have been tested. The blood from eight cases of pneumonia was examined and in each case was the reaction absent.

CONCLUSIONS.

1. The age or sex of the patient does not make any difference in the character of the reactions.

2. The reaction may be obtained from the blood of cases of typhoid fever during the course of the disease, and may be found as early as the seventh day. In some cases it is absent at the seventh day and in other cases it is not fully developed until later in the course of the disease. In no case was it found earlier than the seventh day. In another case it was not perfectly developed on the fourteenth day.

3. The reaction disappears at a varying time after the cessation of the disease. The bacilli may fail to agglutinate as early as the thirty-eighth day. The reaction may be perfect as long as ten years.

4. The reaction can not be obtained from the blood taken from patients suffering from diseases other than typhoid fever, unless there has been a previous attack of the latter disease. Even if there has been a previous attack of typhoid fever the reactive property of the blood may have disappeared.

5. The test is valuable in diagnosing between typhoid fever and other diseases presenting a so-called "typhoid state." This is particularly the case in tuberculous diseases, in catarrhal fever, and in pneumonia.

6. From the observation of the one hundred cases it would seem that the reaction was accurate and that

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it could be relied on. It presents advantages over former clinical methods in doubtful cases.

CASES OF TYPHOID FEVER IN THE DISEASE.

Number.	Age.	Day of Disease.	Reaction.	Diagnosis.	Temperature.	Remarks.
1	25	27	Present	Typhoid fever	105.8	
2	18	18	"	" "	102.8	
3	17	29	"	" "	100.6	
4	26	7	"	" "	103.6	
5	30	17	"	" "	104	
6	13	11	"	" "	102.4	
7	15	11	"	" "	104.4	
8	23	12	"	" "	101.6	
9	16	92	"	" "	102	
10	32	12	"	" "	103	
11	28	14	"	" "	102.6	
12	18	28	"	" "	101.8	
13	20	15	"	" "	103	
14	22	11	"	" "	102.6	
15	26	28	"	" "	99.6	
16	23	30	"	" "	100.8	
17	?	?	"	" "	102	Relapse
18	17	15	"	" "	102.6	
19	40	26	"	" "	102.6	Before bath
20	40	33	"	" "	102.8	
21	29	?	"	" "	104.2	
22	15	20	"	" "	103	
23	5	17	"	" "	103.2	
24	23	?	"	" "	101.3	
25	28	14	Imperfect	" "	102	
26	33	15	Present	" "	102	No bath
27	16	9	"	" "	104.6	After bath
28	22	15	"	" "	104	" "
29	22	8	"	" "	100	" "
30	28	23	"	" "	102.2	Not tubbed
31	23	?	"	" "	102	No baths
32	31	5	Absent	" "	103.2	
33	6	7	"	" "	101	
34	27	19	Present	" "	102.4	
35	31	3	Absent	" "	103.2	

CASES OF CONVALESCENT TYPHOID.

36	18	24	Present	Typhoid Fever	98.8	
37	12	28	"	" "	99.6	
38	22	58	"	" "	98	
39	27	34	Imperfect	" "	99	
40	27	38	Absent	" "	99.2	
41	19	59	Present	" "	98	
42	22	49	"	" "	98	
43	28	30	"	" "	98.4	Relapse
44	28	39	"	" "	103	Temperature due to an attack of tonsillitis
45	16	38	"	" "	98.4	
46	19	87	Absent	" "	97.6	
47	22	104	Imperfect	" "	98.8	
48	28	67	Present	" "	97.4	
49	22	46	"	" "	98.2	
50	31	58	Imperfect	" "	97.2	
51	17	36	Present	" "	98.2	
52	26	62	Absent	" "	98	
53	27	33	Present	" "	97	
54	22	60	"	" "	97	
55	22	31	"	" "	99.2	
56	27	42	"	" "	98.6	
57	16	23	"	" "	98.8	
58	24	27	"	" "	102.8	Temperature due to an attack of meningitis
59	35	105	Imperfect	" "	99	

CASES IN WHICH TYPHOID FEVER HAD PREVIOUSLY BEEN PRESENT.

60	25	. .	Present	Chlorosis	98.4	Typhoid fever ten years ago
61	20	. .	"	Peritonitis	101.2	Typhoid fever in September, 1896
62	20	. .	Absent	"	99.2	Typhoid fever in September, 1896
63	31	. .	"	Healthy adult	98.4	Typhoid fever in 1893
64	26	. .	Present	" "	98.4	Typhoid fever in 1893
65	20	. .	Imperfect	Peritonitis	97.8	Typhoid fever in September, 1896
66	30	. .	Absent	Sequelæ of typhoid	98.4	Typhoid fever in January, 1897

CASES IN WHICH TYPHOID FEVER WAS NOT PRESENT.

Number.	Age.	Reaction.	Diagnosis.	Temperature.	Remarks.
67	21	Absent	Normal blood	98.4	
68	36	"	Axillary abscess	103	Before incision
69	36	"	Axillary abscess	99.2	After incision
70	42	"	Pneumonia	102	
71	25	"	Rheumatism	102.2	
72	28	"	Malaria	98.4 to 103	
73	19	"	Tonsillitis	98.4	
74	19	"	Diabetes mellitus	98.4	
75	50	"	Rheumatism	98.6	
76	40	"	Cerebral tumor	99	
77	20	"	Chronic valvular endocarditis	99.8	
78	22	"	Acute Gastritis	98.8	
79	36	"	Pneumonia	97.2	
80	32	"	Tuberculous pleuritis	99.2	
81	40	"	Pneumonia	99.2	
82	18	"	Peritonitis	99	
83	28	"	Rheumatism	98.2	
84	28	"	Pneumonia	99.6	
85	?	"	Pleuritis	98.4	
86	?	"	Gastro-enteritis	99	
87	?	"	Tonsillitis	99	
88	?	"	Gastro-enteritis	104	
89	12	"	Rheumatism	98.4	
90	33	"	Influenza		
91	18	"	Pulmonary tuberculosis	101.8	Early stage
92	42	"	Nephritis	99.2	
93	?	"	Pneumonia	103	
94	?	"	Pneumonia		
95	28	"	Pleuritis	98.2	
96	31	"	Pneumonia	99	
97	52	"	Catarrhal fever	101	
98	34	"	Pneumonia	100.2	
99	49	"	Nephritis	101.8	
100	13	"	Catarrhal fever	99.4	

The pure cultures of typhoid bacilli were obtained from the Laboratory of Hygiene of the University of Pennsylvania, and thanks are due and are here extended to Dr. Abbott for permission to obtain them and to Dr. L. W. Peckham for making the inoculations.

The specimens of blood which were subjected to the test were obtained from patients in the wards of the Presbyterian and Episcopal Hospitals.

CLINICAL REPORT ON SERO-DIAGNOSIS.

Presented to the Section on Practice of Medicine at the Forty-eighth Annual Meeting of the American Medical Association, held at Philadelphia, Pa., June 1–4, 1897.

BY R. C. CABOT, M.D.

BOSTON, MASS.

I have collected and placed upon the blackboard the statistics relating to sero-diagnosis from all parts of the world, so far as I have been able to get hold of them, including those of good, bad and indifferent technique, and therefore open to criticism. The figures, as you see, show a very large proportion of successful serum tests. Out of 1,826 cases supposed to be typhoid, the result of the serum test was confirmed by autopsy or by the subsequent course of the case in 1,740, or 95.2 per cent. Out of 1,649 cases known to be other than typhoid, the serum reaction was negative in 1,592, or 96.5 per cent. Out of a total of 3,475 cases examined, in 98.8 per cent. the results of the serum test have been borne out by the court of ultimate appeal, the clinician. These are the combined results of seventy-one articles, twenty-five American, twenty-two French, fifteen German, five English and four Italian. About two-thirds of all these cases have been reported by American observers.

My own experience relates to a little over 400 bloods. It has been the experience of the practitioner, and not of the laboratory worker. I have had to do these cases entirely by myself. A large proportion of them have been cases in private practice and I have been able personally to follow every case. In that respect it is a somewhat better series of cases to judge from than those which have been placed under the control of somebody else. My personal results are as follows: In 101 cases supposed to be typhoid I got the reaction by serum in 96. I failed to get the reaction in 5. Of those 5 cases, however, 3 were seen late in the dis-

ease. One was seen only early in the disease and died the day after the reaction was tested. In one case the result was persistently negative throughout; there was never a typhoid serum reaction despite every evidence, except the evidence of autopsy, that it was a case of typhoid fever.

Three hundred and one cases were tested which were known to be other than typhoid fever; 300 were negative, 1 was positive. The single case, which occurred the day before I came to Philadelphia, which therefore I have not had opportunity to follow up, was a case of pernicious anemia in a negro. The clumping was prompt in a 1 to 20 dilution. I have not tried a dilution higher than that. Therefore it does not constitute a surely negative test according to the definition of Professor Welch. Out of 301 cases 300 were negative. The list included every variety of disease such as one sees in the Massachusetts General Hospital, going through the wards at random. It includes a considerable proportion of our epidemic of cerebro-spinal meningitis, seventeen cases of which I have tested, always with negative result. I have taken the blood in almost every case from the ear, using a single drop of blood taken with the medicine dropper and put directly into a small tube containing ten drops of the bouillon culture of typhoid taken with the same dropper. This is not a strictly accurate method, but it seems to me to be sufficiently accurate. It is the quickest and simplest method I know of in that only one drop of blood from the patient is necessary, the puncture is trifling and it can be repeated without troubling the patient as would occur if more blood were taken at each test. The culture is taken in the test tube to the bedside. I have used bouillon cultures, taking care that the bouillon should be as nearly neutral as possible, using stock agar culture transferred once a week; the secondary cultures were either twenty-four hour cultures in the thermostat, or two, three or even four days old kept at room temperature. Bouillon cultures kept at room temperature preserve their motility and apparently their usefulness for this test, for considerably longer periods than if they are kept in a thermostat. And it is also true that the organisms are considerably more sparse, so that the disadvantages of the method mentioned by Dr. Johnston are partially avoided. I have found that the more rapid the motion the more reliable the culture in the technique which I have used. I think it has been very rightly insisted upon by Dr. Johnston, that one man should stick to one technique and work out the advantages and the disadvantages of that technique if he wishes to get the best results. I have not done that myself, but I think that when a man gets a technique he should stick to it in order that he may know the advantages and the disadvantages of his own method.

I have used a dilution of 1 to 10, which, as Professor Welch has said, is now generally thought to be too low a dilution. I think that that question depends entirely on what the length of the time limit is. Most German observers who have condemned the 1 to 10 dilution, have used either no time limit at all or a much longer time limit than that which has been my practice. From fifteen to thirty minutes has been the limit in my cases within which the reaction must occur if it is to be considered typhoid reaction. With the exception of the one case of pernicious anemia, I have met no case other than typhoid in which the typhoid reaction was given by this method. I have used exclusively the quick or extemporaneous microscopic method, never the twenty-four hour method. The testimony of observers who have tried both has been almost overwhelmingly in favor of the quick method, and it has not seemed to me worth while to test the twenty-four hour method considerably.

As to what constitutes a reaction, I agree entirely with what Dr. Johnston has said, that a cessation of motion with clumping is the most satisfactory, and that either clumping without cessation of motion, or cessation of motion without clumping, is to be looked upon with suspicion and the case is to be subject to further test. I think it can not be too strongly insisted on that every culture should be examined every day before a series of typhoid tests is made, with reference to the existence or non-existence of clumps in that culture before any serum is added. This would not be true, perhaps, were the laboratory conditions quite as favorable as possible. If that is done it throws out, I believe, one of the greatest sources of error in this reaction as tried by the general practitioner.

The question arises, How early does this reaction come on? The statistics of different observers are different upon that point. In my own series of thirty cases, in which the patient was seen between the first and the seventh days in bed, twenty-six, or 85 per cent. gave the reaction. One point which seems to me worth discussing is, What constitutes the first day of a typhoid case? I have yet to see any observer who has adequately explained what he means by the first or any other day of typhoid fever. With reference to this reaction, we all know how difficult it is to fix that point. So it seems to me that in reporting statistics we must adopt some fixed rule. I have called the first day in bed the first day of the disease, without meaning that it really is the first day. But, for the purpose of statistics, the first day in bed is as good, or perhaps as little open from criticism, as any other which has been used.

Using that standard, the first day in bed as the first day of the disease, the reaction has been present in the first week in 85 per cent. of my cases. In those reported by Professor Biggs and Professor Park, I believe 63 per cent. was the figure during the first week. In those reported by Elsberg it was only 8 per cent. We need further evidence on this point. Out of the whole number of cases which I have collected there were fourteen in which the reaction appeared very late for the first time, anywhere from the fourteenth day of the disease on to the second week in convalescence. In two of my own cases the reaction was always absent until relapse. It was absent on repeated examinations throughout the whole of the original attack, and present only in relapse. Of the remaining 101 cases it was present sooner than the twelfth day. I have seen the reaction in one case two years after the typhoid attack. There are about sixteen cases on record of these very late reactions, occurring later than eighteen months. The longest period which I have seen carefully recorded was thirteen years.

A source of error which I think has not been referred to is the fact that the reaction is intermittent; that it may appear one day, be absent the next, and present again the third. And this adds to the difficulty of interpreting the meaning of a negative reaction. A negative reaction may mean one of various errors in technique; it may mean that the reaction

has not yet appeared and may appear later. It may mean the reaction has been present but has disappeared.

With regard to the presence of this reaction in the blood of negroes who have not had typhoid fever, a number of observations are on record. Dr. George B. Shattuck of Boston found a positive reaction in two who were not known to have had typhoid, although there was some uncertainty in the history. He found there was reaction in three others whom he was quite positive had not had typhoid fever. In other negroes who had not had typhoid fever he did not get the reaction.

This test is not available for prognosis, in my opinion. The great majority of those who have studied the test have agreed that it is not regularly more marked in severe cases than it is in mild cases. In a general way severe cases tend to have a little surer and stronger reaction, but it is not a rule, and the test can not be used for purposes of prognosis.

A word as to the possibility of this test being used be the general practitioner. I think there is no doubt that, for the greatest accuracy of results, the test should be performed in laboratories by laboratory specialists. But I think it can be performed with *sufficient* accuracy to be a very material help in diagnosis by the ordinary practitioner who has familiarized himself somewhat with the methods of modern bacteriologic work. I think the reaction can be made at the bed-side of the patient, and with a good deal of accuracy if proper precautions are carried out, and I think it is of very material aid to diagnosis, even with the inaccuracies which are necessary when the test is not performed in a laboratory and by a trained bacteriologist.

OPEN DISCUSSION ON THE SEVERAL PAPERS ON TYPHOID FEVER.

Dr. WILLIAM OSLER of Baltimore—The question of meningitis in typhoid fever is an extremely interesting one. Dr. Ohlmacher's[1] is the first communication presented in this country in which the typhoid bacillus has been definitely demonstrated in the meningeal exudation. It is an excessively rare complication. I have never met it personally, and I have records of over one hundred autopsies of typhoid fever. The interesting point is that in neither of these cases reported by Dr. Ohlmacher were there any clinical suggestions as to the occurrence of meningitis. Clinically I have seen meningitis diagnosticated in many cases, but as Stokes pointed out many years ago, there are no definite criteria by which we can make an absolute diagnosis of menigitis in typhoid fever. Just as in the cerebral type of pneumonia, so in the cerebral type of typhoid fever, there may be every symptom of meningitis, even to retraction of the head and jerking of the arms, without one single cubic centimeter of exudate being found at autopsy.

The question of perichondritis touched upon by Dr. Fussel is one of considerable interest, particularly with reference to perichondritis, not internal, but external to which the author did not allude.

Dr. McCormick is still a young man, and I have no doubt that with his years his experience will increase also. There is no good whatever in bringing forward lists of cases without a death. I have had fifty-three cases of typhoid fever without a death under the Brand treatment. My late colleague, Dr. Stewart, at the Royal Victoria Hospital, Montreal, has treated 130 consecutive cases by the Brand method without a death. One thing which I think indicates that Dr. McCormick is a young man is, that he should go on treating typhoid fever by his method when having 19 per cent. of hemorrhages. Had I 19 per cent. of hemorrhages in my treatment of typhoid fever I would change it next year for something much less risky.

As to opening the bowels in typhoid fever, I would like to open the minds of the profession in this country with reference to that point. Take a long series of cases, five or six hundred, take from among them the cases in which there has been constipation, cases not treated with calomel, not treated with intestinal antisepsis, and I will guarantee that the cases which have run the shortest course, which have given the least trouble and the least mortality, are these cases in which there has been constipation. I always hope that if any one special symptom is to develop it may be constipation.

Dr. JOHN CRONYN of Buffalo, N. Y.—Out of sixty-five cases of typhoid which I have seen this winter there was this kind of relapse in four. I would ask Dr. Osler whether the latent poison which redevelops in the patient who is almost a convalescent is in the spleen or glandular structure of the intestine, or somewhere else. Two of my patients were up and about the wards; they lost their appetite, became feverish, and began—this is a symptom to which I wish particularly to call attention—to have a little cough as if they were going into acute tuberculosis. It took them a much longer time to recover after this relapse than it had before to reach convalescence. The treatment used after the relapse was directed entirely toward the spleen and the general system; a mixture of iron and quinin, proper nourishment, followed by ultimate recovery.

Dr. J. M. ALLEN of Kansas City, Mo.—I regard typhoid fever as a self-limited disease, and when cut short it is not the result of medicine but of difference in individual power to resist the germ. There is no other disease in which good, sound medical knowledge and practical experience tells so well as in typhoid fever. I enter protest emphatically against the calomel purgative course of treatment. I believe there is something in intestinal germicide remedies, but I have not seen that this or any other system of treatment will cut short this disease. I desire to endorse Dr. Osler's statement because as you may know, I can go back to the period when it was considered safe to let the bowel remain closed, if you please for a week, and the percentage of recoveries in that class of cases was always large. When there is constipation it carries with it a positive idea that the pathologic condition is limited. Hence the chances for recovery are better. I do not mean to say that the patient should not have an action of the bowel once in twenty-four hours, but I do wish to say that there is not the slightest necessity for his having more than one action in twenty-four hours. The general plan of treatment should be one of practical common sense; one case with water, another with something else, using all the remedies which have been mentioned, according to circumstances. There is no place that the physician realizes the statement of the great Dr. Watson more fully than here, that in obviating the tendency to death he has treated the case masterfully.

Dr. FOWLER of Pennsylvania—I am almost amused at Prof. Osler's criticism of Dr. McCormick's paper. The Professor says that if he had 19 per cent. of hemorrhages from any plan of treatment he would abandon it as soon as possible. I would take this ground: If I had a certain number of cases of typhoid fever of such severity that nineteen of them had hemorrhage, and severe hemorrhage at that, and they got better in spite of it, I would stick to my plan of treatment notwithstanding.

Dr. HERRICK of Cleveland—It is obvious to me that there are two classes of inquirers with regard to this disease. One comes to the disease with the microscope in the hand, the microscopist who sees it with that instrument. Another class includes the macroscopist, who sees the coarse characteristics of the disease. Most of the gentlemen here, I presume, come to this subject as the macroscopist. Now, the facts are that this is a disease of an organ, and there is no question but what that organ has a peculiar relation to the whole system, and that relation gives rise to the phenomena which we observe in typhoid fever. It is an enteric fever. We are all agreed as to that. Whether it is due to an infection, or whether the infection comes subsequent to inflammation of the glands is a matter of some interest. I will not discuss that. I am ready to accept both theories. The infectious theory is a working hypothesis for sanitation. I would take all precautions against infection. There is inflammation of Peyer's glands. What is the danger from that? There is danger of sepsis being carried through the tumefied glands into the general circulation. Let us apply common sense to the treatment. I can approve of the treatment advised in the several methods in some instances, for there are no two cases alike. You can not classify the disease as you can the plants of a garden. I would urge the necessity for holding to our physiology and anatomy in their application to this disease. Call it infection, call it inflammation, but there is a local infection, and there is no fever until that local infection has been carried into the circulation. All agree on that point.

As to treatment, it should be as simple as possible. The digestive organs are the ones affected. There is no better principle to carry out in inflammation than rest. Put the patient in bed. Put the digestive organs absolutely at rest. Do not

[1] Paper recently received, will be published very soon. [ED. JOURNAL.]

fill up the bowel with milk, or meat, or those things that constantly irritate the bowel. With regard to medication, I do not care if it is only water, pure water, and given abundantly outside or inside. I prefer it inside. I do not believe in Brand. I do not believe in being chained down to Brand's method, or Woodbridge's method, or anybody else's method.

Dr. ANDREWS of Pennsylvania—I should like to counteract the influences of the statement made by Dr. McCormick that opium should not be used in hemorrhage of the bowel in typhoid fever. I trust the medical profession will not lose sight of that leading principle in surgery as well as in medicine, that the first requirement in all cases of hemorrhage is absolute rest of the part. It is just as necessary to arrest peristalsis where hemorrhage occurs, or seriously threatens, as it is to quiet the heart in hemoptysis.

Dr. J. E. WOODBRIDGE of Cleveland, Ohio—The gentleman who read the paper did not follow my advice, or did not quote me correctly in every instance. I certainly never said that one dose would cure a patient. One gentleman says that reports of cases are of no value. How, then, are you to establish any theory in medicine? If in one epidemic they have a certain percentage of deaths, and in the following epidemic they have no deaths by a different method of treatment, is that not some evidence that the different method is better than the one first employed, especially if the duration of the disease is greatly shortened? If in the hospital in Massachusetts they had a very large death rate, they changed the method of treatment and had no deaths, and the same physician who applied the treatment in the hospital also applied it in private practice and had no deaths, quite a large number of cases having been treated, is not that some evidence that the one method is better than others, especially when all his brother practitioners using former methods had the same death rate as in previous years? Such instances could be multiplied indefinitely. The cases are numerous.

I have used Professor Osler's name, and I fear he has misunderstood the manner in which I have used it. I wish to say that I used it in the most complimentary way. I took him as the representative of the best thought of the medical profession of the United States and of the world when I quoted him. I have been accused of saying many harsh things, but I want to say that I never said a harsh thing about any member of the medical profession until I was driven to it by harsh duty. Leading members have continued to write me and ask about my management of typhoid fever. I would like to read one letter from a gentleman of wide experience, who said he proposed to write me about my views and apply the method as closely as his intelligence would permit, and if he found that I had misrepresented facts, if he found I did not give him a correct statement he would denounce me before every medical society in the land. He writes me that since commencing the treatment of typhoid fever by this method he has employed it exclusively and he would not now use any other. All of the patients recovered; none had sequelæ. In one the temperature went two or three degrees higher within two days after beginning the treatment, but it then came down and the patient made an early recovery. That is only one letter out of perhaps fifteen hundred which I have received from various physicians. If I am wrong, there are a great many physicians in the United States who are wrong. The gentleman whose letter I have quoted has charge of a hospital through which thousands of cases pass annually; he knows typhoid fever when he sees it, and he would have denounced me if he had not secured the results which I had promised.

As to one remark of Dr. Osler—I beg his pardon—that cases of typhoid with constipated bowels did better than those with diarrhea. The diarrhea of typhoid is evidence of poison, and if the patients are treated by the antiseptic system there can be no diarrhea, but every single case will be constipated. If antiseptics are properly used it is not possible to have diarrhea, or to get along without using saline laxatives in addition to the little tablets.

Dr. OSLER—Could you not have diarrhea from fermentation?

Dr. WOODBRIDGE—In the antisepticised alimentary canal you can not have diarrhea. You will have constipation in spite of all you can do, and you will have to fight that from the beginning to the end of the disease. I can give these tablets and get up passages next day; I may get them the day following, but not on the fifth, sixth, seventh or eighth day. You can not give enough of the tablets for such an effect. In one hospital it was said they were given ten at a dose, yet it was necessary to give Hunyadi water. Those are only a few facts.

Dr. ——— —I have looked into the Brand system, and have been much pleased with the results. It is applicable to the hospitals in our large cities. Now we need something which can be applied by the country practitioner in houses where there is no bath tub. If we can get just as good results from the Woodbridge method, or the McCormick method, let us have both of them tried. I feel that the professors in our great hospitals would do a very great service if they would put these several methods on trial side by side in our great hospitals, and announce the result for the benefit of the general practitioner. It is a notorious fact that physicians are so prejudiced against the antiseptic treatment that they are afraid to give it a trial.

Dr. MCCORMICK—It would appear to be a piece of presumption on my part, being but a country doctor, to read a paper here that would in any way lead this Association to believe that the college professors and the makers of text books were not teaching the proper teaching. But, somewhere early in my education as a physician, I conceived the idea that the cure of disease was the ultimate object and aim of the physician, and on that basis I have practiced medicine ever since. So far as the hemorrhage of typhoid fever is concerned, I wish to say to my friend, Professor Andrews of the Medico-Chirurgical College, that no stronger impulse to abandon opium was ever made upon my mind in case of hemorrhage in typhoid fever, than when I was called in consultation by a young man who had just left his college to see a woman who had had hemorrhage and had lost considerable blood. The doctor gave this woman a hypodermic injection of a quarter of a grain of opium. At the end of fifteen hours she had another hemorrhage and he gave her another injection. After this I saw her in consultation. She was swollen like a drum. I said I thought I would not have given her the second dose. He replied that he was never taught anything else. That woman died, and I am as firmly convinced as I have existence that she would not have died if she had not received that injection of morphin. If one single principle has been demonstrated in medicine it is that opium and morphin are no longer the agents with which to treat inflammation in the abdominal cavity. The surgeons long since taught us that we must stop giving opium in these diseases, and if we must stop giving opium in surgical diseases, why not more so in medical diseases? I believed the college professor was infallible; I believed the text books were infallible. But when my patients died under the treatment they told me to give them I made up my mind something was wrong.

Dr. OSLER—As to college professors and authors of text-books, they are just as fallible as the rest, just. But they have eyes and ears, and some of them common sense, and those who have common sense when they have a case of typhoid fever which is bleeding will give opium and morphin.

SUMMARY OF VIEWS EXPRESSED AT THE DISCUSSION ON SERUM DIAGNOSIS AT THE MEETING OF THE AMERICAN MEDICAL ASSOCIATION AT PHILADELPHIA.

The committee appointed by the Chairman of the Section on Practice of Medicine make the following report:

1. In selecting the material used in making the test the choice between: *a*, serum, *b*, dried blood, *c*, fluid blood, and *d*, blister fluid, will depend largely upon whether the object be scientific research, clinical diagnosis in hospital or private practice, or public laboratory diagnosis where the samples have to be sent some distance.

2. In spite of considerable variation in technique, there has been a remarkable uniformity in the results obtained by those taking part in the discussion, and their average of about 95 per cent. of successes agrees with the general average of the cases, nearly four thousand, thus far recorded in medical literature.

3. Each of several methods of technique advocated may thus give good results in the hands of those thoroughly familiar with the details found necessary in each case and the sources of error to be avoided, success depending rather on being perfectly familiar with one method than on the particular one selected.

4. For routine diagnostic work even the very simplest methods may give good practical results, but for recording scientific observations those methods which are accurately quantitative should be selected. This is especially necessary in reporting exceptional cases at variance with the general results recorded or where the observations are made the basis of generalizations.

5. A complete reaction should comprise both characteristic clumping and total arrest of motion occurring within a definite time limit. For practical diagnostic work a dilution of 1 to 10, with a fifteen minute time limit, is convenient. In any doubtful case the dilution should be carried as far as 1 to 50 or perhaps 1 to 60, and a reaction not obtainable at that point should

not be regarded as perfectly conclusive. For these higher dilutions the time limit should be extended to two hours.

6. Intensity of reaction in a given serum should be estimated by determining the degree to which it may be diluted without losing its power of giving a decided reaction, both as to agglutination and loss of motion.

7. The intensity of reaction shown by the same serum is influenced by the age, condition and virulence of the test culture and by the composition and reaction of the culture medium. For purposes of comparison the sensitiveness of the test culture should be taken into consideration.

8. The evidence so far recorded establishes that the reaction may be delayed or occasionally may not be obtained in cases of genuine typhoid infection; and also that it may be exceptionally present in non-typhoid cases, though not in an intense degree.

9. In investigating exceptional and contradictory results the following circumstances have to be considered: *a*. The uncertainty of clinical diagnosis. *b*. The absence of bacteriologic or other confirmatory methods of diagnosis during life, giving decisive negative results. *c*. The possibility of overlooking typhoid infection even postmortem, in the absence of characteristic intestinal lesions where a very thorough bacteriologic examination has not been carried out.

10. The modifying influences mentioned above suffice to explain the divergencies existing in the reports of different observers. Without being absolutely infallible the typhoid reaction appears to afford as accurate diagnostic results as can be obtained by any of the bacteriologic methods at our disposal for the diagnosis of other diseases. It must certainly be regarded as the most constant and reliable sign of typhoid fever, if not an absolute test.

N. B. The above summary, while expressing the general consensus of opinion brought out during the discussion on serum diagnosis before the Section on Practice of Medicine of the AMERICAN MEDICAL ASSOCIATION, does not claim to represent exactly the individual views of any one of those who took part.

W. H. WELCH, WYATT JOHNSTON, J. H. MUSSER, R. C. CABOT, S. S. KNEASS, A. C. ABBOTT, J. M. SWAN, E. B. BLOCK, H. M. BIGGS, N. S. DAVIS, JR., M. W. RICHARDSON, J. B. HERRICK, A. R. GUERARD, A. P. OHLMACHER, Committee.

NEUROLOGIC PROGRESS IN AMERICA.

Presented to the Section on Neurology and Medical Jurisprudence, at the Forty-eighth Annual Meeting of the American Medical Association, at Philadelphia, Pa., June 1-4, 1897.

BY C. H. HUGHES, M.D.

ST. LOUIS.

America has contributed much to the creation of modern neurology, especially to neuriatry or clinical neurology. By the term neurology I include normal neurology and abnormal neurology, which I term neuriaty, and psychology and its opposite morbid aspects psychiatry. In this great historic city one of its most honored names in medical and political history, Dr. Benjamin Rush, was the pioneer psychiatrist and neuriatrist[1] (if you will permit the coinage of this word) of the century. The consideration of mental aberration received much attention at his hands. It was here that insanity first had a hospital set apart to its victims, and inebriety was treated as a disease under the name oinomania. This great physician and statesman made a study of alcoholism, its causes and consequences and boldly proclaimed it the drink disease in advance of all the world, just as Ephraim McDowell in Kentucky had boldly cut into the abdominal cavity and made the first successful laparotomies, thus opening the way to the greatest of the present life-saving triumphs of modern surgery, as he had also opened the way for that great declaration of principles that made this a free independent and invincible nation.[2]

No record of American medical progress, no tribute to American medical glory is complete without Pennsylvania, and no literary monument commemorative of the the profession's grandeur and honor could stand whose pedestal is not placed in this city of medical greatness and brotherly love. He not only took a seat in the Continental Congress and made a preliminary draft of and signed the final Declaration of Independence, but he laid the foundation, with his eminent colleagues, broad and deep for the first, and still living and vigorous, American medical college. But he did more in neuriatry than I have mentioned. He recognized the neuropathic diathesis which he called the "phrenitic predisposition," "a union of diffusive morbid irritability involving the nerves and that part of the brain which is the seat of the mind," and he notes the disposition and capacity of certain lunatics to simulate sanity for a purpose. He also notes how the incipient delusions of the insane are mistaken for the cause and relates a case of delusional mental infidelity in a woman "of great medico-legal significance" (*op. cit.*, p. 44). He recognized and named a phase of affective or moral insanity before the term had been invented in what he called the lieing disease.

Rush's definition of illusion, "a waking dream," is the briefest on record, and his amplification of it, as "a false perception, in the waking state, from a morbid affection of the brain," is perhaps the best on record. His presentations of the morbid aspects of love, grief, joy, fear, anger, reverie, the moral faculties and, in fact, of all the passions and of the morbid state of the sexual appetite, are unique and advanced for his day; described in 1793 a sexual pervert who answered the doctor's question as to desire and excess thus: "*Dixit per annos tres, quinque vices, se coitum fecisse in horis viginti quatuor, et semper semine injecto*," antedating the records of Arnold, Krafft-Ebing and others on this subject.[3]

He was familiar with sexual tabes dorsalis, hypochondriasis, etc., and from 1804 to 1807 records four cases of madness due to onanism. He took the psycho-somatic view of insanity, such as the best alienists hold today, saying that "did cases of madness reside exclusively in the mind, a sound state of the brain ought to occur after nearly every death from that disease," whereas he know of "but two instances upon record of the brain being found free from morbid appearances in persons who have died of madness."

Here is sound modern psycho-pathology and a correct psycho-somatic conception of the nature and definition of insanity such as we hold today. He denied that insanity was "an ideal disease" and asserted that "epilepsy was a bodily affection."

He had a correct conception of aphasia as a brain trouble, though he neither knew its precise seat nor its name as we call it today. Although he believed too much in venesection, as we believe too little, his

[1] I certainly think the time has come for the invention and use of this term. A term I have employed for many years in my lectures, explaining its derivation and necessity.

[2] Though not germane it is a source of professional pride to note the historic fact as a "member of the provisional conference of Pennsylvania and chairman of the committee to which was referred the great question whether it had become expedient for Congress to declare independence," he made a "report which was adopted and sent to the Congress the same day." This was a "most animating document, most probably written by Rush, as he was chairman of the committee and ever ready with his pen." The whole committee consisted of himself and Col. James Smith, and "included all that has been so much praised in the declaration attributed to Thomas Jefferson, of which it might appear to have been the protocol" (Stone's "Eminent American Physicians and Surgeons." p. 437). Rush also went into the Continental Congress, knowing he was to vote for independence, for he was elected to fill a vacancy created by the resignation of a member who declined to remain and vote for separation.

[3] Vide p. 548, "Diseases of the Mind," Philadelphia, 1812.

therapeutics for acute violent mania, venesection, solitude, seclusion and rest, acts of justice and a strict regard for truth, "everything necessary to their comfort and every promise made to them faithfully performed," is good *fin de siècle* pyscho-therapy, except that we have therapeutic substitutes for blood-letting not known to this archiater in physiologic medicine. He was the American pioneer in the modern management and treatment of the insane and the world's pioneer in correctly understanding and providing for the inebriate as a brain-damaged man. He was the world's champion of the mobid inebriate's rights and demanded for him hospitals for treatment like other sick persons. "They are as much objects of public humanity and charity as other people," he said (*vide op. cit.*, p. 267). He invented the tranquilizing chair as a more humane substitute for the straight jacket, and his law of kindness with his patients is now the modern rule of action for all hospitals for the insane. Rush's record might be further extended, but enough has been said to name him among the immortals in medicine and philanthropy as he is among statesmen. His name was not born to die whether Congress builds or neglects to honor Washington with his statue.

Surely the profession of America and especially of this great city should be proud of having had in its ranks this far-seeing philanthropic physician.

Before leaving this subject we here note the fact in parenthesis that Robley Dunglison, also a Philadelphian, in earlier editions of his classic medical dictionary, has informed us that such a nervous condition as nervous debility existed and even gave us the name of neurasthenia with the long accent on the i.

The first systematic treatise devoted exclusively to the medical jurisprudence of insanity in any language was written by the friend and the benefactor of my youth, Dr. Isaac Ray of Philadelphia, a work still recognized as high authority in American and foreign courts. Ray's exposition of the clinical features of insanity are clear and concise. His delineations of its affective forms have never been surpassed, while his pleas for justice to the insane have been so masterly and forceful as to markedly modify the practice of American courts in the direction of greater humanity and justice to the mentally maimed than prevailed before his day. He so amplified and emphasized the definition of Andrew Combe: "A prolonged departure without adequate external cause from the natural modes of thought, feeling and action of the individual," as to make it plain and effective in many celebrated cases of lunacy inquiry before American courts and juries. His clear-cut description of morbid mental character as shown in the Parish and Angell will cases, the Hinchman and Bernard Cangly cases, and the trials of Rogers and Winnemore won him undying forensic fame as a faultless psychiater in court, while his analysis of Shakespeare's delineations, his illustration of the insanity of distinguished writers and of King George III.; in short, every chapter in his contributions to mental pathology prove him to have been the peer in power of psychologic analysis of the mind diseased, of any alienist of his day. His treatise on mental hygiene for youth further confirms this opinion, and I had opportunity in his later lifetime to personally observe his clinical ability as a very Corypheus in practical psychiatry as I, as a guest, walked the corridors of Butler Hospital with him while he filled the place of my friend, Dr. Sawyer of Butler Hospital, then absent in Europe. Both are jewels in psychiatry's crown. Ray was the author of "The Propositions," a set of rules and regulations for the government of hospitals for the insane, which, like his little treatise on the good superintendent and the good attendant, were never equaled before and have not been surpassed since.

Since Ray, Galt, Stearns, Awl and Gray, Godding, Spitzka, Hammond, McLane Hamilton, Fisher, Webber and Folsom have made valuable contributions, also Storer and Mann, and I myself have added a few monographs, but none have surpassed or even equaled the great leader whose mortal remains lie buried in Greenwood, while immortal memories stretching over the world of psychologic science, center at his tomb.

Before passing on I stop to note the most philosophic treatise on pseudopia since "Dendy's Philosophy of Mystery," or "Brierre de Boismont's Hallucinations" appeared, and far more scientific, namely, "Visions: A Study of False Sight" (pseudopia), by Edward H. Clarke, M.D., of Boston, written, like the memoirs of General Grant, Napoleon and Thomas H. Benton, while fighting malignancy, marching remorselessly and resistlessly to inevitable victory. In this book, written in 1877 and dedicated to Dr. Oliver Wendell Holmes, the author's anatomic, physiologic and pathologic basis of cerebral visions still holds to a very considerable extent with the profession and though not wholly novel at the time, no work up to that date with which I am familiar, had so fully or so clearly presented the subject.

We go back now to an earlier date to find in neurologic annals the distinguished name of Dr. Amariah Brigham, first of Vermont, later and finally of New York, and his two treatises, the one on "Mental Cultivation and Excitement," in 1845, the other on "The Brain and Nerves," show how early and forcibly American attention was drawn to neurology as a proper and imperative study. In an inquiry of 227 pages, "Concerning the Diseases and Functions of the Brain, Spinal Cord and Nerves," published by George Adlard, New York, in 1840, by this author, is contained a great many valuable clinical contributions to psychiatry and neurology, which have not been duly credited to him. He was the first alienist to look upon masturbation in certain cases as the result and not the cause of insanity, contrary to Rush.

The power of expectant attention and self introversion is most intelligently presented by him under the head of "Effects of Mental Attention on Bodily Organs," giving clinical illustrations of dyspepsia, melancholia and fever thus induced and of cures effected. His original view of dyspepsia as a nervous breakdown, I believe to be the most tenable view of the subject in the face of extensive clinical observation, as I have elsewhere noted.[4] He noted cases of hysteria in the male and elucidated the subject, as well as of chorea, tetanus, hypochondriasis, delirium tremens, etc. His conceptions of the nature and causes of neuralgia were nearly up to the present date. He understood epilepsy about as well as we do today and enjoined that "the brain and nervous system of those who die of epilepsy should be examined with great care, and whenever opportunity presents should be compared with those who have never been affected with that disease." He had advanced knowledge re-

[4] Vide Address on Medicine, American Medical Association, at San Francisco, 1894.

specting the relationship of the vagus nerve and sympathetic systems. Discussing a case of suicide in a case of recurrent delusional insanity, which, on post-mortem examination, showed "upon the nervus vagus, or pneumogastric nerve, of the left side, just before the recurrent is given off, a fixed hard jagged body about the size of a kidney bean or small wart composed of calcareous matter," he says: "When we consider that the nervus vagus rises in the medulla oblongata, but is chiefly distributed to the great organs not under our control, and that it communicates with almost the whole of the ganglionic nerves, we may form some idea of the irritation and disturbance produced in the digestive, sanguiferous and sanguific organs by a jagged calcareous mass implanted, as it were, into one of the most important nerves of the great vital viscera!"[5] How like a modern neurologist he speaks! But hear him further. Concluding an article on the great sympathetic nerve and diseases and autopsic morbid appearances connected therewith, with regret at the paucity of knowledge of its functions, he says: "We know . . . enough to convince us that they (its functions) are important" and "we can not but lament its condition is so little regarded in disease and in postmortem examination."[6] Tilt could talk no better. He discusses focal lesions of the brain and of all of the cranial nerves in a most intelligent manner for his time; gives a case of traumatic aphasia without, of course, having a name for it, and a case of bullet wound of the corpus callosum from Hennan with apparent recovery, but subsequent death from a bout of drinking with the wounded soldier's comrades. But we can not follow this interesting pioneer in neurology further. From what we have thus far taken from this author we are well prepared for such up-to-date expressions as appear in the preface to Brigham on the brain, notwithstanding they were spoken fifty-seven years ago. "The study of the human brain yields in utility and dignity to no other. It is the study of the most important part of the organism, of that portion for which all the others seem to be created. It is of the highest philosophic interest from the connection of the nervous system with the manifestation of mental phenomena."

"From the general diffusion of this system and its known uses we should expect it to have great influence in disease and that, as intelligence and mental cultivation, the excitement of the feelings and passions, all of which affect this system, increase, that an increase of nervous diseases and new affections of this system should be observed." And this we find to be true. Apoplexy, palsy, inflammation of the brain, dropsy of the head, insanity, etc., are far more common now than in past ages and are most observed in countries where there is the most mental excitement. We also now witness forms of nervous diseases or affections of the brain and nerves that were nearly unknown half a century since. Enjoining a more accurate knowledge of the anatomy of the brain and its connections, he regrets that "generally diseases of the nervous system are not thus investigated," and complains that "attempts to elucidate the diseases of the spinal marrow by autopsic examination are quite rare and those of the ganglionic system still more so." "Every one knows," he continues, "that certain symptoms indicate disease of the brain, or its membranes, or nerves, but until quite recently, and now almost generally, physicians rest satisfied with merely knowing that disease is located somewhere in the skull, but we should certainly strive to know more than this. . . Few physicians are satisfied with merely knowing that a patient has some disease within the abdomen or thorax; they seek to know what particular organ is affected and what tissue or portion of the organ is diseased. . . . We have but to pursue the same course as regards diseases of the nervous system," etc. "This field of investigation is indeed very great and will require for a long time many laborers, but ultimately, I apprehend, will richly reward those who cultivate it."

The object of his work, he tells his readers in his preface, "is to call attention of practitioners of medicine to the importance of the nervous system; and to persuade them to embrace every opportunity to study its functions and diseases," which have "vastly increased with the increase of civilization and now constitute a far greater proportion of the diseases of mankind than in past ages and consequently demand far more attention."

Here was the neurologic pathfinder who blazed the way through the unknown forests of neuriatry for an army with torches and banners whose bright lights have since illumined the world. By these early lights we now see why it is that American neurology has so many brilliant and sturdy devotees and contributors like its emblematic statue of Liberty, to enlighten the world. If nervousness is, as our neurologic cousins across the water assert, the American disease, it had American discoverers, and the name of George M. Beard is immortal as a later pathfinder in neurology and neuriatry. His untimely death was indeed a great loss to the world and that part of the world in which we delve.

His treatise supplemental to "Neurasthenia" entitled "American Nervousness," and the "Scientific Basis of Delusions" and other contributions which go to make up the pedestal of his fame, are noted elsewhere.

M. Gonzalez Echeverria, whose work on epilepsy with anatomico-pathologic notes, original plates and engravings, first appeared in 1870, had about as thorough a clinical knowledge of epilepsia as any writer of his time, and basing his views on clinical observation and the experiments of Kussmaul and Tenner, "that sudden arterial anemia of the brain, as also Faradization of the cervical sympathetic nerves which determines permanent spasms of the blood vessels, gives rise to epilepsy," he maintained that epileptic convulsions are likewise induced, as well as "when the blood rapidly assumes a venous character."

Kussmaul and Tenner's experiments, in addition to those of other physiologists, establish the chief share which the sympathitic has on the determination of spasms and confirm Brown-Sequard and Vander Kolk and *he* maintained that the seat of epilepsy "the *nodus epilepticus* is in the medulla oblongata, with a material modification in every case." The over-excited action of the "ganglionic cells of the medulla controlling the vasomotor elements and nutirtion," were to him the seat and cause, the invariable and necessary element, of the epileptic paroxysm which he says, "may well occur without muscular spasms." Notwithstanding the exception taken by Vander Kolk to Echeverria, "cerebral anemia is among the very initial phenomena of the epileptic paroxysm."

Though the exclusive medulla oblongata origination of epilepsy is not now tenable and cortex or Jackson-

[5] P. 222. [6] P. 197.

ian epilepsy is a proven fact, yet the vasomotor spasm view of this author has not yet been overthrown and his "Discussion of the Modern Doctrines of Epilepsy" is still critical and classical in the light of further and later discovery.

Next upon the scene appear in paths to American Neurologic glory William A. Hammond, Weir Mitchell, H. C. Wood, Seguin, E. C. Spitzka, McLane Hamilton, James G. Kiernan, Chas. K. Mills, J. J. Putnam, Isaac Ott, Charles L. Dana, R. W. Amidon, M. Allen Starr, J. K. Eskridge, S. V. Clevenger, B. Sachs, Daniel R. Brower, Harold N. Moyer, Frederick N. Peterson, William Fuller and a host of others whose names may come to us as we proceed, and their work appear in the supplement to this paper and in the future historian's pantheon of America's neurologic gods.

Hammond, by his indefatigable zeal and captivating style of writing, gave an impetus to neurologic medicine whose wave is still felt in the profession. In 1867, shortly after his retirement from the surgeon generalship of the United States army, he founded and for many years maintained the *Journal of Psychological Medicine and Medical Jurisprudence*, a high grade quarterly periodical which was promptly accorded and always held front rank in the literature of neurology.

In the second volume, the dynamometer and dynamograph are for the first time, in this country at least, illustrated and elaborately described by Dr. H. In this volume Hammond maintains that chlorosis is a disease of the nervous system. In this volume also appears a unique presentation of the subject of "Carnomania" by Charles F. Taylor; the "Microscopical Appearance of the Brain and Spinal Cord" by J. G. Webber of Boston; "Historical Considerations Concerning the Properties of the Roots of the Spinal Nerves" by Austin Flint, Jr.; Roberts Bartholow, then of Cincinnati, and E. C. Seguin have separate articles on "Aphasia;" T. Edwards Clark handles the subject of "Animal Magnetism;" Hammond, "The State of the Mind During Sleep;" "Organic Infantile Paralysis," S. Henry Dickson;" "The Legal Consequences of Insanity," Horatio R. Storer; "The Law of Rape, Suicide and Insanity," by T. Edwards Clark; Hammond, "Epilepsy Due to Cerebral Anemia;" Nathan Allen of Lowell, "The Law of Human Increase, or Population Based on Physiology and Psychology;" "Statistics of Opium poisoning" by Alonzo Calkins of New York; "An Improved Pocket Æsthesiometer" by Hammond, besides an interesting chronicle in each number of the world's contributions to psychiatry, neurophysiology and neurology. This ably conducted and invaluable journal was continued for years.

Hammond's classical works on "Diseases of the Nervous System," "Insanity," etc., are too familiar to the neurologic world to require elaborate portrayal here, even if we had the space and you the time for the record.

Hammond's early impress on American neurology, as it is upon the literature of medicine, is indelible. His original description of athetosis alone is an addition to neurologic discovery, as are Weir Mitchell's on "Erythromelalgia" and the cremaster reflex that have stood the crucial test; and I think I may likewise modestly claim something for the virile reflex and its clinical and medico-legal value when it shall have been further studied. The difficulty of eliciting it is its chief obstacle to acceptance, but it is an undoubted clinical fact of great value in diagnostic neurology.

Hammond inspired a host of younger men to follow his footsteps. In January, 1874, under his inspiration the first number of the *Chicago Journal of Nervous and Mental Diseases* was started by the lamented and talented J. S. Jewell and his able associate H. M. Bannister; the first article of the first volume being an original lecture on the "Pathology of the Vaso-motor Nervous System" by Jewell, who had produced the same at a lecture in the Chicago Medical College. These lectures were continued through this volume. The articles, selections and annotations were of a high order. This journal was temporarily suspended at the death of its talented and cultured senior editor, but soon revived again and brought out in New York, where it continues to appear as a monthly of no mean merit under the editorial management of Dr. Charles Henry Brown and an able corps of well-known collaborators.

In January, 1880, my own journal, the *Alienist and Neurologist*, first appeared in St. Louis. It has never missed an issue since its first number and we continue to do business at the old stand and on the same principles of progress; regarding a proper understanding of the nervous system, anatomic, psychiatric and neuriatric, as the chief end of man in medical research and practice.

Antedating all of these periodicals, we must not omit mention of that staid and valuable old neurologic periodical, limited in its scope, however, to the psychiatric side of neurologic medicine, the *American Journal of Insanity*, which, under the editorial management of Richard S. Dewey of Chicago, continues the good work so well begun over a half century ago. In its earlier issues may be found the records of American clinical and forensic psychiatry of most of the old masters, especially of the original thirteen who founded the Association of Superintendents of American Hospitals for the Insane, from which has descended the present American Medico-psychological Association. Their names were: Samuel B. Woodward of Connecticut, first president of the association and originator of the Hartford Retreat; Samuel White, of the same State, and late Professor of Obstetrics and Surgery in the Berkshire Medical Institute, founder in 1830 of a private institution for the insane on the Hudson, and president of the New York State Medical Society; Isaac Ray; Luther V. Bell, of New Hampshire; Charles H. Stedman, of Massachusetts, a graduate of Yale and Honorary of Harvard; John S. Butler, of Hartford; Amariah Brigham; Pliny Earle; Kirkbride; William M. Awl, of Pennsylvania; Francis T. Stribbling, of Virginia; John M. Galt, of Virginia; Nehemiah Cutter, of New Hampshire—all men who made their mark well for the welfare of the insane and the advancement of American alienism.

No country ever had so enthusiastic, united and powerful a phalanx to fight for the rights of the insane. Following in the footsteps of Chiarugi, Pinel, York and Tuke, they bravely fought the foes of psychologic advance to overwhelming defeat and won a lasting triumph of humanity and science in the treatment of American insane.

Weir Mitchell's work is likewise too familiar to us all to need recalling here. He is known out west as "rest cure" Mitchell, and "fat and blood" Mitchell, though he himself at the time he was so christened was as lank and lean as hungry Cassius. But his

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fame rests on more enduring laurels, though these were enough. His views of the cerebellum as a re-enforcing ganglion, first offered in 1869, have lately been reaffirmed by Italian physiologists.[6] Besides what I have already referred to his scientific, to say nothing of his literary work, beginning in 1852 with an investigation into the "Various Forms of Uric Acid Crystals and their Alterations in Highly Acid Urine," and not yet ended, we hope, in his instructive clinic lessons of the present year, is a rich heritage of inestimable value to America's and the world's contributions to medicine. Hysteria, mental aberration, sciatica, neuralgia, neuritis, sleep jerks, ataxia, headaches and apparitions, blood pressure, double consciousness, the reflexes and too many other subjects to here enumerate, make up a volume of titles alone for the instruction of the student of neurology that will perpetuate his name forever.

The elder John K. Mitchell, father of Weir, is claimed by the latter to have antedated Sir William Gull in the description of spinal arthropathies, and the present John K. Mitchell, grandson of the senior, is walking wisely in the footsteps of his father. His recent book, "Remote Injuries of Nerves," is a valuable presentation of the subject highly creditable to modern American neurology. The younger Mitchell has added something worth considering to the classic work of Bowlby to the study of section and injuries of nerves and made valuable contributions on ascending and migratory neuritis and the knowledge of contusions and commotions of nerves, and degeneration and regeneration thereof has been advanced in his recent work, "Remote Consequences of Injuries of Nerves."

No history of neurasthenia would be complete without record of the writings of my much esteemed friend, Dr. E. H. Van Deusen of Kalamazoo, Mich., whose observations on this subject made public in 1867, I called renewed attention to in the initial volume of my journal, the *Alienist and Neurologist*, in 1880. Dr. Van Deusen was at the time medical superintendent of the Michigan Asylum for the Insane, and under the caption, "Observations of a Form of Nervous Exhaustion (Neurasthenia) Culminating in Insanity," wrote as follows: "Our observations have led us to think that there is a disorder of the nervous system, the essential character of which is well expressed by the term given above, and so uniform in development and progress that it may with propriety be regarded as a distinct form of disease."

A reference to the article introducing the subject will show also the essayist's view on that subject.

Neither would the subject of inebriety be justly treated without reference to the many American contributions to its literature by Mason, Crothers, Wright and others. Dr. T. D. Crothers has contributed since 1875 over a hundred articles on this subject, his chief elucidations being of alcoholic trance states and the medico-legal aspects of inebriety.

Alcohol and drug addiction and their psychic and physical causes have been studied by Crothers, Mason, Matteson and many contributors for the *Journal of Inebriety*, a quarterly periodical which has long held the uncontested field as the only journal of the world devoted exclusively to this subject. While some of its editors' views have been regarded as too radical, others have undoubtedly been held as advanced. Its clinical records give it just claim to a prominent and permanent sharer in neurologic progress. Valuable contributions by Gurdon W. Russell, T. L. Wright, Mason, Crothers and others, may likewise be found in the earliest numbers of my own journal. Wright's Treatise on this subject had its inception in the earlier contributions to the *Alienist and Neurologist* as Geo. T. Stevens' later book on "Eye Strain in Nervous Diseases" did. In 1877 Stevens enucleated an eye-ball and cured a case of diabetes and in 1888 another for epileptiform disease with diabetes. Both recovered.[7] The subject of oculo-neural reflex irritation also first appeared in the (*vide supra*) *Alienist and Neurologist*.

The foundation of the American Medico-Psychological Association which was begun by the original thirteen as the Association of American Hospitals for the Insane and the organization of the American Neurological Society was the beginning of the diffusion of correct conceptions of mental and nervous diseases and their real clinical and medico-legal significance among the general profession and the people. This work has also been greatly promoted by the New York, Philadelphia and American Medico-legal Societies, and sections and societies of psychic research. The clinical lectures of E. C. Seguin gave the world new light on the spastic paralyses and paraplegia, the excellent work of Spitzka takes rank with that of Golgi and others abroad on the fine anatomy of the brain. While Sepilli was working on cerebral thermometry in Italy, Amidon and Carter Gray were engaged in the same work in New York and Bert Wilder has overwhelmed us with an almost entire recast of the nomenclature of cerebral anatomy, some of which has already and much more is destined to be finally accepted by anatomic cerebrology.[8]

The late contributions of William Fuller of Grand Rapids, Mich., to the objective study of course cerebral anatomy by his unequaled sectional models and book of plates, as well as his contributions to neurosurgery, tapping the lateral ventricles and exploration for cerebral abscess, the differentiation between coma of compression and reflex arteriole spasm coma, in 1897, his enunciation of the anemia theory of convulsions, his arteriole spasm theory of the treatment of cerebral convulsions and his cranial sections for certain forms of idiocy at an earlier date, certainly entitle him to honorable mention in any record of neurologic progress. He also maintains that keloid is of the nature of neuroma and caused by injury or disease of nerve endings, and he drained off the subarachnoid in a case of convulsions, embarrassed respiration and cerebro-spinal fever in 1880.

In 1879 Dr. C. H. Hughes read before the Missouri State Medical Association a paper entitled "A Clinical Inquiry into the Significance of Absent Patellar Tendon Reflex," showing at that early date that the knee phenomenon was absent in many cases other than locomotor ataxia. Subsequent or simultaneous observations of others, including Landon Carter Gray, Bannister, Erb, Westphal and Tschirjen, have confirmed the fact. The paper maintained that the diagnostic value of the patellar tendon reflex sign was rather in its having been lost in connection with other ataxic symptoms. Dr. Gray was the first to maintain that the nerves involved in the tendon reflex phenom-

[6] vide American Journal of Medical Science, Vol. 57, p. 320.

[7] See Alienist and Neurologist, 1, Vol. No. 1.

[8] The old world and ourselves may make wry faces at having to swallow the new nomenclature, as chemistry did a few decades ago, but I think we shall have to come to Wilder's terms, not all of them, but most of them in time. Wilder schemes than his have been made practicable in science.

enon belong to a variety not hitherto recognized, viz.: voluntary nerves with involuntary fibers distributed to voluntary muscles, while Gowers at the time objected to characterizing the phenomenon as periphero-central. But it is quite probable that all voluntary muscle innervation has also latent involuntary nerve fiber tracts, and that the involuntary muscles have also latent voluntary fibers, else how are we to explain the phenomena of convulsions and the control of the heart in certain persons, as in the case of Colonel Townsend, who could at will arrest his, and the control of the ears, scrotum, etc., in others.

Peripheral nerve tissue is readily reproduced. Dr. S. Weir Mitchell, in speaking of the pathologic results of neuritis after injury to nerves, long ago noticed an enormous development of connective tissue elements, and Herdman has made some confirmatory observations the present year on nerve repair after injury. But the reproduction of brain substance has been doubted. Al. M. Vitzoy[9] (Bucharest) found in the brain of a monkey a new formation occupying the back part of the skull, after incision of the occipital lobes two years previously, which he proved to be nerve cells and neuroglia exactly comparable in appearance to those present in normal tissue. Theodore Simon, in the earlier editions of Virchow's *Archives*, reports some cases of what he denominates additional brain growth where new formations were found superincumbent upon the gray matter of the convolutions. In these new growths the gray and white matter were normal in their relations and proportions. But Dr. John B. Gray as early as 1875 (Tranactions of the New York Academy of Medicine) records two cases of reproduction of brain tissue after brain injury, the length of time in reparation of tissue corresponding with reparation of nerves after injuries. The brain was some twenty days in completing its structure and the convolutional character of the surfaces was distinctly marked.

The case of Phineas P. Gage, who had a part of his left brain destroyed by a premature quarry blast in Vermont, and the projection of a tamping iron through his head, Sept. 13, 1848, subsequently dying on a farm near San Francisco, May 20, 1861, is the most remarkable contribution to the subject of brain tolerance of violence and the possibility of brain repair on record. The iron that went through his head was a cylindric bar one and one-quarter inches in diameter, three feet seven inches in length and weighing thirteen and a quarter pounds. The bar tapered to a quarter of an inch and was chisel-shaped at one end. It entered the brain in front of the left lower jaw, small end first, and out through the anterior-superior part of the left parietal, destroying the anterior part of the left hemisphere, thence through the corpus callosum anteriorly to the opposite side, extensively involving the margin of the right hemisphere, lacerating the anterior and middle lobes, falx and the longitudinal sinus. Gage recovered by the sixty-second day so that he walked a half mile, and by the seventy-third day so that he went home thirty miles away, the wound being closed on the one hundred and twentieth day. This man traveled, exhibited himself and the bar, acted as a hostler, drove a stage coach in South America and worked as a farm hand, dying of epileptic convulsions after an irregular life, in which intemperance played a part, but without paralysis or mental impairment, nearly thirteen years after; that is, he had neither paralysis nor mental impairment in the beginning. I was told by Dr. Warren or Dr. Bowditch, at the Boston General Hospital, by whose courtesy I saw the skull and tamping iron in 1868, that Gage was actually intellectually a brighter man after than before the accident. We all know that the skull of this remarkable case is now in the Warren Anatomical Museum and that the man was never lost sight of by American devotees of psychologic and neurologic science from the time of his remarkable recovery until head and history were secure in the depositories of science for the good of mankind. It will always be regretted that the brain could not have been examined microscopically. But psychiatry owes to Boston a debt of everlasting gratitude for what it has preserved in its archives of this case under such difficulties, as it likewise is indebted to the labors of C. E. Brown-Sequard for his contributions to the duality of the brain and the bromid treatment of epilepsy, though an English surgeon timidly antedated him before he took up his abode in Paris, and America antedates the world on the subject of brain and nerve repair and on the subject of neuritis. Kiernan, in 1882, and myself have contributed something to the literature of the duality of the hemisphere of the brain. I saw at the same time another remarkable case in the Boston General Hospital from Findlay, Ohio. I think it was a young man who had blown through his head, through the pontal region, a piece of gas piping about three-fourths of an inch in diameter.

Since Hammond, Allen McLane Hamilton, Landon Carter Gray, Wood, Dana, Ranney, M. Allan Starr and Dercum have given us treatises that compare with the best foreign authors. Dercum has marshaled in his treatise a galaxy of bright particular stars of the neurologic firmament, and Chas. K. Mills has in preparation a volume which we predict will be unsurpassed in any language. These, with Fuller's "Architecture of the Brain" already referred to, Shaw's "Diagnosis," Clevenger's and Moston's, would seem almost to have amply supplied the demand for neurologic literature without the many excellent translations. But the "Twentieth Century Practice," an international encyclopedia of modern medical science by leading authorities of Europe and America, edited by Thomas L. Stedman, M.D., New York city, is before us, and here is what an able reviewer[10] says of one of its volumes to the credit of American neurology:

"Volume ten is devoted to 'Diseases of the Nervous System.' With the exception of Dr. Féré of Paris, who furnishes articles on hysteria, epilepsy and the spasmodic neuroses, the contributors to this volume are all our own countrymen—the international element is less in evidence than in the preceding volume.

"Dr. Joseph Collins of New York has a long article of 300 pages on the important subject of diseases of the brain, and another on diseases of the meninges. Dr. B. Sachs of New York deals with tumors of the brain, and is sanguine enough to predict even far greater success in the diagnosis and surgical treatment of intracranial neoplasms during the next decade than has been achieved in the recent past. Dr. Charles L. Dana of New York writes on neurasthenia; Dr. H. T. Pershing of Denver on disorders of speech, and Dr. Sanger Brown of Chicago closes this volume of 859 pages with a short article on disorders of sleep."

[9] American Journal of Medical Sciences, October, 1865.

[10] Boston Medical and Surgical Journal.

Treatises like those of Wilks, Ross, Maudsley, Bevin Lewis, Obersteiner, Hirsch, Erb, Westphal, Edinger, Mendel and others, with Charcot, Sepilli and a host of other German, French and Italian confrères, will ever be esteemed in America, but it is now plain that if cut off from them, American neurologic medicine would not starve for neurologic nutrition in its psychic centers.

But let us proceed. The first medico-legal differentiation between aphasia and aphasic insanity in this or any other country, so far as I can discover, was made by myself in 1879. The first contribution on the "Simulation of Insanity by the Insane" was also written by myself in 1876. In 1880 I wrote on nitrite of amyl in differential diagnosis, on reflex cardiac gangliopathy; in 1881 on consciousness in epilepsy, in 1882 on hyoscyamin and something new on the diagnosis of neuratrophia or neurasthenia. I proved the vasomotor contractile power of cephalic galvanization in 1883, maintained and established the curability of certain hitherto hopeless types of epilepsia and showed that the lesion of hemophilia and malarial hematuria is in the sympathetic system. While it is perhaps true that

> "To observations which ourselves do make,
> We grow more partial for the observer's sake,"

and that we do not wish our work, real or fancied, for the weal of the world forgotten, we must not overlook the fact that we have but meager space here for detail record of any one's work, so I pass over a few dozen of my own contributions to further notice those of your distinguished confrères in neurology, noting some of their work in the context and the remaining in an appendix.

Let us make a short trip to Chicago and dwell a few moments with that living neurologic and psychologic encyclopedia, James G. Kiernan. Next to Kohlbaum, he has thrown more light on katatonia than any other writer in the country. His first contribution on this subject appeared in 1877. And next on the study of Shakespeare's psychiatric characters, while his psychology and psychiatry of the prominent characters of history has not been exceeded by John C. Buchnill or William Ireland. His contributions to the study of psychology and psychiatry of genius, and the neurology as contradistinguished from the neuriatry of genius, have been numerous and instructive. Insanity in nearly all of its various forms and in many novel features has been described by his fountain pen of neurologic truth. In 1882 he wrote on the duality of the cerebral hemispheres.

The relation of insanity to crime has been instructively studied by Harold N. Moyer, Kiernan and others in this country; insanity proceeding from the colon by the former and by the lamented Jewell; the medical jurisprudence of railway surgery and shock by Moyer, Clevenger, Outten; the nervous sequelæ of influenza by Moyer, myself and others; a rare form of occupation neurosis by Moyer; also paranoia, nystagmus, acromegaly, infantile chorea, neuritis with knee jerks and nystagmus, exophthalmus with nephtis, etc.

In an inaugural thesis before the American Neurological Society, entitled "A Study of Nerve Cells and their Functions," Clevenger appears to have anticipated the later neuron theory (*vide* Chicago *Medical Review*, March 11, 1881, and "Comparative Physiology and Psychology," 1885). Fry has made some recent studies on the same subject. Clevenger's contributions are too numerous for detail here.

Frederick N. Peterson in 1879 made valuable studies on the posterior cerebral lobes in an inaugural thesis, and has kept up his search for new neurologic light steadily ever since. Among his later contributions are his prize essay on "Morbus Basedowii," the principles of craniometry, the study of muscular tremor, cataphoresis, electrothanesia, the colonization of epileptics, deformities of the hard palate in degenerates, the new phrenology, katatonia (with Dr. C. H. Langdon), chapters in Starr's special forms, Starr's children's diseases, Loomis', Bigelow's treatises, and the American text-book of diseases of the nervous system.

The "State Hospitals Bulletin" of New York, conducted by Wise, Brown and Reeves, has thrown a flood of new light over the pathway of our progress, Ira Van Giesen having made some startling revelations from the State Pathological Institute, of which he is director, on the relation of the auto-intoxications to neural diseases, which Nelson Teeter has followed up and elaborated on the autotoxic origin of epilepsy. Teeter's clinical studies on cerebral tumor and Courtney's on pachymeningitis also grace the first number of the first volume.

While trephining for cerebral pressure by fluid was first suggested abroad by T. Clay Shaw in 1889, and performed by Hamson Cripps in July of that year, it was done in this country on the following March by Wagner of Utica.

In 1891 Quincke first performed paracentesis spinalis for hydrocephalus. In May, 1896, Turner performed this operation for general paralysis, and in this country Dr. Warren L. Babcock in July of same year; and these bulletins contain a record of his work. Here, too, we find Hutchings following up the discovery of Huchard on cerebral meiophragias with valuable researches on this condition of arterio-sclerosis associated with certain mental symptoms.

In truth, these bulletins reveal a bewildering scope of work creditable to American clinical psychiatry and neurology, and we can not name all the works save in a bibliography. Here Somer's case of general paresis in its postmortem showing coincides with those of the West Riding Asylum's early reports, and Babcock's moral insanity confirms the contentions of our own earlier days.

Syphilitic hypochondriasis was presented in 1888 by Allen McLane Hamilton, and Carter Gray has lately added to the diagnostic signs of melancholia and intracranial syphilis, and Frank C. Hoyt of St. Joseph, Mo., pathologist to State Lunatic Asylum No. 2, gives us postmortem light on this subject.

Hugh T. Patrick, who is no more of a saint than my friend who dreams over the hookah, though he hails from the saintly city of Chicago, maintains that he has knocked out the Bryson symptom in exophthalmic goiter, showing by a study of forty cases that the diminished chest expansion frequently found in Graves's disease is in no sense pathognomonic, but is simply an expression of the general myelasthenia which he maintains is always present in this affection. An experimental and anatomic study of the course and destination of Gowers' tract by this writer, established the fact the Gowers' antero-lateral tract extends as a separate bundle as high as the corpora quadrigemina and then passes in a retrograde direction to the middle lobe of the cerebellum.

The intimate relation between asthenic and bulbar

paralysis and poliomyelitis, has been shown by this author in a recent report of a case of so-called polioencephalitis superior and inferior with a careful microscope. Dr. Patrick's studies in trunk anesthesia in locomotor ataxia and syringo-myelia reveals a novelty in symptomatology and he maintains that "spinal irritation" is psychic and not located in the spine.

Frank G. Lydston and Eugene S. Talbot have made valuable contributions to the study of the stigma of degeneration especially in criminology, including regicides, inebriates and aristocratic degenerates. Chicago is a good place in which to study criminology and degeneracy and these gentlemen are well qualified for the work.

M. Allen Starr's atlas of the nerve cells, with the coöperation of Strong and Leaming, though published abroad, is highly creditable, like Fuller's plates and casts, to American neurologic industry, ability and ingenuity. Starr's work on brain surgery has been complimented by translation into the German and French languages. His contribution to the study of tumors of the spinal cord, 1895, and diagnosis of cerebral abscess, 1897, are valuable additions to cerebrology and spinology.

We are indebted to Daniel R. Brower of Chicago, for a new surface thermometer, and many studies in medico-legal and traumatic insanity, electro-therapy and spinal neurosis; to Brower, Andrews and Hughes for separate clinical studies of hyoscyamin; to Brower, Clevenger, Kiernan and many others, for records of traumatic insanity; to Brown for the case of Mark Gray (concealed insanity) and Pendergast as a paranoiac, and too many other records and monographs for our space.

Progress in the knowledge of the localization of sensations has been made in this country, especially through the studies of Dr. Charles L. Dana, and the diagnosis of intra-cranial hemorrhage and acute softening, vertigo in temporal lobe lesions; apoplexy, the apoplectic pulse, the cause of perforating necrosis of the spinal cord, its continued sclerosis, the pathology of chorea and paralysis agitans, and studies in alcoholism, acromegaly and the pathologic anatomy of tic douloureux, besides his text book on nervous diseases, now going into the fourth edition, which has features unexcelled in any book published abroad.

The literature of multiple neuritis, paramyo-clonus multiplex, the neuron conception of the nervous system, chorea and Raynaud's disease have been written upon by Fry, of St. Louis. Bauduy has given us a book that ought to be revised; Bremer has added to the literature of microscopic blood states in disease, and Shaw's diagnostic neurology and contributions to morbus Thomsenii, are valuable additions to the literature of neurology, and my own work which appears mainly in my journal, the *Alienist and Neurologist*, founded in 1880, offer to your critical consideration. If I have accomplished but little it is not because my intentions have not been good. In addition to what has already been cursorily referred to, I claim the introduction of chloral hydrate per rectum in puerperal eclampsia, infantile convulsions and obstetric practice, besides early articles on the dual action and vicarious functions of the cerebral hemispheres and lobes of the brain, aphasia, hyoscyamin in psychiatry, moral and other forms, and the definition of insanity. If we take the wings of the morning and fly to the uttermost parts of the earth we shall find the neurologist there, so rapid has been the progress of neurology within the short time since it began to take rank as a medical specialty, almost within the average life of a generation of men. But we read and go beyond the confines of our own country to be satiated with the richness of neurologic contributions. An examination of the psychiatric and neuriatric bibliography here appended would make one feel, after what we have thus far gone over, like "gnawing a file and fleeing unto the mountains of Hepsidam" for rest, "where the lion roareth and the whangdoodle mourneth" for our diversion, but we will stop awhile at the Rockies and here we find Pershing perched on the heights of Denver, Thombs lower down in Pueblo, and Jeremiah T. Eskridge, the lion of the tribe of Judah, who went out to Denver with one lung and developed a voice that has been heard in highest and strongest neurologic notes around the world. He has been so active there that the festive bacillus tuberculosis could not find further lodgment on his never-resting cerebral anatomy. I have before me a record of 103 contributions to the literature embracing every aspect of neurologic inquiry from diagnosis, pathology, physiology and treatment to the neural therapy of climate. His articles on retro anterograde amnesia, temporary abulic agraphia, symptoms of speech disturbances as aids in cerebral localization, on brain tumors and glioma, and chapters on insanity and feigned diseases, the latter in the "American System of Medical Jurisprudence," have commanded our attention.

Since American medical literature had its birth here and since we started our incursion here it is mete that we return. There are neurologic giants in Philadelphia whose measurements we have not yet taken. The contributions of Dr. James H. Lloyd embrace brain tumors, syringomyelia, diseases of occupations, the spinal cord in pernicious anemia, Friedrich's ataxia, etc. And here are some of the blows Chas. K. Mills has struck in opening the way to the high pinnacle on which American neurology stands today:

"The Relation of Infectious Processes to Mental Disease," *American Journal of the Medical Sciences*, November, 1894; articles in an "American Text-book of Diseases of Children," edited by Louis Starr; "The Naming Center," *Journal of Nervous and Mental Disease*, January, 1895; "The Localization of Lesions in the Pons and Pre-oblongata," *International Clinics*, vol. iii, fifth series, 1895; "Some Phases of Syphilis of the Brain," *Medical News*, vol. lxvii, 1895, p. 606; "The Diagnosis of Intracranial Tumors," *University Medical Magazine*, March 18, 1896; "Mistakes in Neurological Diagnosis," *Philadelphia Polyclinic*, July 25 and Aug. 1, 1896; "A Case of Cerebral Abscess Situated at the Posterior Part of the External Capsule," with Dr. William G. Spiller, *Journal of Nervous and Mental Disease*, September, 1896; "Cases of Aphasia Illustrating Especially Disorders of Pantomime," *Philadelphia Hospital Reports*, vol. iii, 1896; "A Series of Reports of Cases from the Neurological Department," *Philadelphia Hospital Reports*, vol. iii, 1896; and "Treatment of Diseases of the Brain," in an American Text-book of Applied Therapeutics edited by J. C. Wilson, 1896.

Dr. Mills has nearly completed the first part of a "Practical Treatise on the Nervous System and its Diseases," a volume of about 1,000 pages, which will be issued by J. B. Lippincott Company of Philadelphia, about the first of October of the present year, and every American neurologist will be proud of it. He is the author of too many neurologic monographs

to be here enumerated; his articles, clinical and pathologic, include reports on many cases of brain tumor; numerous clinical lectures and reports on the affections of the nervous system; articles on hypnotism; medico-legal papers; the Toner "Lecture on Mental Overwork and Premature Disease Among Public and Professional Men," published by the Smithsonian Institution; articles on hysteria, hystero-epilepsy, catalepsy and ecstacy, in the "American System of Practical Medicine;" and in the same work, "Tumors of the Brain and its Envelopes" (with Dr. James H. Lloyd); numerous reports on cases of insanity and papers on cerebral and spinal localization, and "Cerebral Localization in its Practical Relations."

Besides these names those of Spiller and Marineso come to me, but a record of the work must now be deferred.

As I close this report I note that the leading articles in many of the latest issues of the leading journals of the country are on advanced neurologic subjects. Lewellys F. Barker in the *New York Medical Journal*, continues his interesting contributions on the nervous system and its constituent neurons; Allan Bonar in the *Record*, gives us new light on many disturbances in locomotor ataxia, and Henry Hun, who has contributed much in years gone by, gives an instructive study of analgesia, thermic anesthesia and ataxia, from focal softening in the medulla oblongata and cerebellum due to occlusion of the left inferior posterior cerebellar artery, further enlightening us on the course of the sensory and coördinating tracts in the medulla, and Henry M. Lyman of Chicago, in the JOURNAL OF THE AMERICAN MEDICAL ASSOCIATION for May 22d, elaborates the subject of nervous dyspepsia with proofs of the position taken on this subject (though he does not name them as such) by Brigham seventy years ago and referred to in the beginning of this essay.

During the past year there has appeared in my journal alone advanced neurologic articles on the following subjects: "The Anastomoses Between the Spinal Accessory and the Vagus," by Drs. D. Mirto and E. Pusateri; "Some Current Errors Regarding Insanity," by Dr. Arthur E. Mink; "A Case of Chronic Chorea, with Pathologic Changes Similar to those of General Paresis," by Dr. E. D. Bondurant; "A Note on the Treatment of Sexual Inversion," by Havelock Ellis; "The Advancement of Psychiatry in America and the Relation of Psychiatry to General Medicine," by Dr. Edward Cowles; "Abuse of the Bromides," by Dr. Harriet C. B. Alexander; "An Ataxic Paranoiac of Genius," by Dr. J. G. Kiernan; "Hysterical Analgesia," by Dr. C. C. Hersman; "State Care and State Maintenance for the Dependent Insane in the State of New York," by Dr. Carlos F. McDonald; "Nervous Shock and Disease of the Nervous System as a Cause of Pernicious Anemia," by Dr. James B. Herrick; "Differential Diagnosis of Insanity," by Dr. C. B. Burr; "Observations on the Histologic Development of the Cerebellar Cortex in Relation to the Faculty of Locomotion," by Dr. Aurelio Lui; "Scrivener's Palsy not Solely Pen Fatigue," by Dr. C. H. Hughes; "Are Americans Degenerates?" by Dr. Jas. G. Kiernan; "Sociology and the Realistic Novel," by Dr. Ingeborg Taustrom; "The Surface Thermometry of the Head in Diseases of the Brain," by G. W. McCaskey; "Syphilis as an Etiologic Factor in the Production of Locomotor Ataxia," by Dr. C. Travis Drennen; "The Psycho-Neural Factor in Medical Practice," by Dr. C. H. Hughes; "Psychical Hermaphroditism," by Dr. William Lee Howard; "Preputial Reflex Epileptiform Convulsions, with Report of a Case," by Dr. Alex L. Hodgdon; "Intemperance, Consanguine Marriages and Educational Overpressure, as Factors in the Genesis of Nerve Disease and Degeneration of the Race," by Sir Frederick Bateman, M.D.; "What is Meningitis?" by Dr. W. S. Christopher; "The Case of Sturgeon Young, a Question of Hypnotic Injury and Death," by Clark Bell, Esq.; "Encephalitic and Late Epilepsy," by Dr. J. G. Kiernan; "Psychoses of Old Age," by Harriet C. B. Alexander; "The Auto-toxic Origin of Epilepsy," by Dr J. Nelson Teeter; "Insane Heredity," Dr. H. P. Stearns; "Analgesia of the Ulnar Nerve in the Insane," by Dr. Arrigo Giannone; "Report of a case of Brain Syphilis Heroically Treated with Mercury, Followed by a Mercurial Neuritis and Recovery," by Dr. William C. Krauss; "Interaction of Somatic and Psychic Disorder," by Dr. James G. Kiernan; "Imperiative Conceptions," Dr. C. H. Hughes; "Defence of Modern Psychiatry," Dr. Wm. Hirsch; "Cyclone Neuroses," Dr. C. H. Hughes; and the Effect of Extirpation of the Parathyroid Glands," by Prof. G. Vassale and Dr. F. General; while the list for the past ten years is a complete record of neurologic progress.

It is thus that American neurology and psychiatry moves up to the mountain top. If you do not concede that it is already there, you see we are getting there. The labors of the past three decades, as well as the work of the pioneers, have won for us a place at least beside our worthy brothers abroad in the world's neurologic progress. We at least are lending a helping hand, of which we need not be ashamed, in making its history. We are moving up with the world and moving the world up with us.

NOTE—The author wishing to make the report complete would be obliged for further facts with accurate bibliographic references.

SOME OBSERVATIONS AND EXPERIENCE IN THE TREATMENT OF EPILEPSY ACCORDING TO THE METHOD SUGGESTED BY DR. FELIX VON NIEMEYER.

Presented to the Section on Practice of Medicine at the Forty-eighth Annual Meeting of the American Medical Association, held at Philadelphia, June 1-4, 1897.

BY MATTHEW WOODS, M.D.

PHILADELPHIA, PA.

So much has been said and done within the past few years in attempted elucidation and cure of that complex series of phenomena commonly known as epilepsy or "fits," that the preparation of another paper on the subject might be deemed an impertinence, but for the fact that the disease and its treatment, judging from printed current opinion on the matter, is still as much of a problem as it was twenty years ago, and that the treatment I am about to elucidate produces better results than that of any treatment with which I am acquainted.

It is not the intention of this paper, nor is it at all necessary to my purpose to discuss the cause and nature of the disease—a sort of "Pigrogromitus of the Vapians" among diseases, no one seeming to know who or what they were—so of epilepsy. The old theories are familiar enough, yet so little still is known of its etiology and pathology, that correct classification of its various forms is almost impossible. The familiar terms, *grand mal*, *petit mal*, traumatic, central, idiopathic, peripheral, congenital, laryngeal, gastric, Jacksonian, post-paralytic, hereditary, acquired, imbecilic, genito-neuropathic, senile, nocturnal and the like, *ad infinitum*, are mere pedantries of nomenclature bearing but slight relation to its causation factors and throwing no light upon the origin of the disease. One of the many merits of the treatment, according to the effective way suggested by Niemeyer, consists in the fact that, combined with various adjuncts, it is about equally efficacious in traumatic and non-traumatic epilepsy, in the variety due to organic disease where the spasm is but a symptom, and also in what may be called true epilepsy, where, as far as we know, the convulsion *is* the disease.

It is not likely that any one is able to tell much

that is new about the malady, except perhaps a few of the less conservative of surgeons who, by certain brilliant work and at infinite pains, have shown, as it would seem, the possibility of curing or relieving certain cases by certain surgical operations on the brain, but unfortunately at such a risk, that it seems a question if the rather meager results were sufficient justification for the great danger.

And yet the frequent failure of operation on the brain for the cure of otherwise incurable epilepsy, is not an argument against the operation itself, but rather a confession of the inadequacy of our knowledge of its pathology to explain its phenomena and characteristics. Surgical operations on other parts of the body have been a benefit to epilepsy, the cure probably being due to shock, just as shock will sometimes induce an epileptic attack, and I am convinced that the operation on the brain would have more frequently relieved the malady, if it had not been for the many dangers involved in the operation itself.

The normal contents of the cranium completely fill, we are told, the cranial cavity. Any foreign material, therefore, entering or developing in this cavity, such as tumors, projection of bone from a depression or fracture consequent upon a blow, extravasated blood, exudation of cicatricial tissue, accumulation of pus, lymph or serum, necessarily diminishes the containing capacity of the calvarium in proportion to their bulk, and consequently destroys the equilibrium between the brain and spinal cord. When an epilepsy therefore is known to be due to this sort of pressure, and there is no other remedy, and the last state of the man is not likely to be worse than the first, in consequence of the operation, it might seem as if an attempt should be made to remove the cause.

But are we ever sure that epilepsy is due to mere pressure alone, and that removal of the apparent cause, even when such a procedure is comparatively safe, will leave the patient well? Is there no foundation for the theory that epilepsy is a habit to be cured by moral suasion, diet and medicine, rather than by the knife.

Men have had tumors of the brain without epilepsy. Cranial exostoses consequent upon specific disease have existed without fits; patients with exceedingly thick skulls, a thickness not congenital and consequently diminishing the normal containing capacity of the cranium, are not necessarily epileptics. Depressions of the skull due to unskilful use of obstetric forceps and other accidents, such, for example, as a crowbar going through the brain, as is well known, are not invariably the cause of fits, while men with sound heads and free from depressions are sometimes victims of the disease.

Lord Byron, whose brain was so large in proportion to the cranium that the mental activity consequent upon composition caused pain, was not an epileptic, while Cæsar, Hannibal, Mohammed, Charlemagne, Napoleon, with none of the stigmata of degeneracy, at least as far as sculpture and painting have recorded them, evidently were at the same time hydrocephalic, and other idiots with enormously distended crania, and nothing much in them but water, are often subjects of epileptic spasm.

It would thus seem that the presence of certain conjectural causes in the brain are not in themselves sufficient to justify operative procedure when the danger of removal is so great.

I am thus particular to mention the surgical treatment of this malady, first, because that in the modified Niemeyer treatment I have been using so many years with such gratifying success, surgery sometimes forms an exceedingly important adjunct to the treatment by drugs, and, secondly, because too much importance has been attached to the brain as the seat of epilepsy, and pressure on the brain as its cause.

The Briggs case is one in point. The patient, an epileptic, had a depression of the cranium due to fracture and at the same time a tibia requiring surgical aid. "The surgeon first wisely operated on the shinbone, leaving the skull, the more likely cause of the disease, untouched." For five years after the patient had not had an attack of epilepsy. An experience which might mean that in this case the seat of the disease was the shin, but it might imply also that *shock* had something to do in modifying that excessive reflex excitability, perhaps the principal medium of seizure, and that on that account, not because the operation was done on any particular place, the cure was effected.

The seat of epilepsy may be anywhere, perhaps there are some parts more frequently its site than others, and any treatment not recognizing its versatility in the matter of starting point, must often fail. Its origin may be the cerebellum, the convolutions of the brain, disease or injury of the cerbro-spinal centers or of their meninges. Its origin may be in the mucous membrane of the bowels, the stomach, the sole of the foot, the finger, the os uteri, or any peripheral or central part of the nervous apparatus where an irritant may exist, and if there is essential analogy between man and the lower animals, which the writer decidedly doubts, to justify conclusions beneficial to man on observations made on these sometimes lower creatures, we might be led to believe it possible for epilepsy to be induced by simple friction of the skin covering the ribs, and even by the swallowing of such a usually innocuous fluid as common writing ink.

We once had a maltese cat that developed unmistakable petit mal, epilepsia meteor, that is to say, loss of consciousness with the muscular contractions limited to certain sets of muscles, by merely stroking her sides; and a tame raven, a resident of our home and constant companion for a year, would be seized with a complete epileptic attack, epilepsia gravior, immediately after drinking ink, of which he was fond. In the raven's attacks all the symptoms of grand mal (except pallor) appeared in their natural order, including the "epiletic cry," in this case a croak, universal clonic convulsions lasting however only a few minutes, then cessation of the fit, coma, apparent fatigue and sometimes sleep, after which he would remain entirely well until he indulged in another drink of ink. I have allowed the bird to thus contract as many as three fits a day to gratify the scientific and idle amusement of friends, but finally stopped it because of apparent cruelty.

A dose of quinin given to a patient being cured by the proper administration of bromid of potassium, will bring on an attack, so also will a large dose of the bromid itself not sufficiently diluted in water. The administration of tinct. muriate of iron and of the scales in solution of the pyrophosphate of iron, will cause the return of epilepsy after the disease has been apparently cured.

The pain in the back so frequently complained of in anemia, and which our laparotomy friends usually ascribe to some obliquity of that modern scapegoat,

the over-vilified womb and its unfortunate appendages, is perhaps mainly due to gastric spasm caused by undigested food, occasionally to uterine and ovarian disturbance acting in a reflex manner.

This hyperkinesis or abnormal motor activity of the muscular tissue of the digestive organs due to vicious diet is, I believe, a much neglected cause of epilepsy. Nocturnal attacks are frequently brought about by an undigested supper; and abuse of the digestive organs, the cause of so many other disorders, is perhaps largely to blame for epilepsy.

I have seen epilepsy cease with the cure of an endometritis, also twice with the repair of a lacerated os. I also saw a case apparently cured by an operation for a congenital epispadias, the operation an adjunct, like the above, to the Niemeyer treatment. The epispadias likely having but little to do with the epilepsy since it preceded it a number of years, the shock of the operation or the impression made on the mind by preparation for the operation so altering the excessive reflex power—evidently the cause of epilepsy—that the seizures ceased. I have also had an inveterate case of the disorder, one but slightly benefited by drug treatment, cured by having her hand so charred in a frying pan that it had to be amputated, after which she had no more attacks of epilepsy up to her death, which occurred four years subsequent from consumption.

I mention these experiences because to the lessons they have taught me in connection with the persistent administration of bromid of potassium in gradually increasing doses and in large quantities of water, the two conditions insisted upon by Niemeyer, I may ascribe the success I have had in the manage- of this popularly supposed incurable disease.

I am convinced that the failure of this drug in the hands of so many physicians to cure most cases of epilepsy, and help all, except cases where there is congenital deformity, is due to the lack of persistence in its administration, to not recognizing the necessity of giving it largely diluted in water and according to a regularly graded increase in the quantity prescribed, and also because I am convinced that the failure of the drug in the hands of so many is due, not to its inefficiency, but rather to its improper administration—the importance of its dilution in large quantities of water not being recognized. Even such a one as Brown-Séquard is so careless as to say, in talking of epilepsy: Give 20 or 60 grains of bromid of potassium four times daily, as the case may be, omitting to mention the quantity of menstruum, not apparently knowing that the salt will bring on the attack, if given in concentrated solution, and that its efficacy in causing cure and amelioration is due to the attenuation in water that facilitates endosmosis and causes a non-irritating assimilation.

The amount given depends entirely on the severity and frequency of the attacks, from 10 to 60 or 90 grains from one to four times daily. The treatment should be continued for at least a year after the cessation of convulsions and in prescribing this drug it should be remembered that a faulty administration will bring about a recurrence of the attacks.

In addition to the treatment by the drug, resort to the surgeon should be had as occasion requires. Lacerations of the os and other ills of the male or female genitalia should be remedied; constriction of the prepuce in epileptics ought to be corrected; constipation should not be allowed to exist; difficult menstruation cured; suspicious cicatrices should be excised; indigestion should be avoided; the adoption of a suitable dietary is of the greatest importance, allowing but a limited quantity of animal food and but once daily. Epileptics ought to be employed out of doors when possible, and in employment bringing a reward. Distension of the stomach by undigested, fermenting food and impaction of the bowels are a prolific cause of nocturnal attacks. An important point in the treatment of the disease is attention to the mouth, as deformities of the palate, alveolar abscesses, necrosis and exfoliation of bone are rather frequent and demand care. All the natural functions should be carefully protected from abuse.

By attending carefully to the above details and others less important perhaps, that my limit precludes me from mentioning and by giving the drug strictly in the way I have indicated, joy, hope and health may be restored to many so-called incurable epileptics, at present abandoned to despair, and many whose existence consists merely of suspense and unconsciousness may be rescued to lives of useful industry, and protected from attacks against which they suppose medicine can oppose no barrier.

DISCUSSION.

Dr. H. A. WEST of Galveston, Texas—Just before I left Texas I saw a case of about the severest petit mal in a child which ever came under my observation, and I wish to refer to some unusual reflexes in that case. The child had a very long prepuce which I desired to amputate, but the parents would not allow it. In addition it had a persistent and long continued hemorrhage from the bowel. Thinking that this might have had something to do with the epilepsy, I directed the use of a solution of subsalphate of iron in the rectum, and also gave bromid of sodium and sulphate of codein. The father told me that the hemorrhage subsided and the number of epileptic attacks was reduced one-fourth or one-third. It is evident the reflex disturbances had to do with the epilepsy in that case.

A CASE OF RENAL TUBERCULOSIS.

Presented to the Section on Practice of Medicine, at the Forty-eighth Annual Meeting of the American Medical Association, held at Philadelphia, Pa., June 1-4, 1897.

BY J. DUTTON STEELE, M.D.

PHILADELPHIA, PA.

The following case occurred in the wards of Dr. Joseph Hearn in the Philadelphia Hospital. I am indebted to him for the clinical history.

L. H., 23 years of age, a native of Ireland, white and married. There is no tubercular family history. She was admitted in October 1896. The patient had measles in childhood. Influenza five years ago which left her with a "weak right side." In January 1896 she was delivered of a six months fetus and since that time has been confined to bed continually. She has become much emaciated and has a profuse diarrhea. Examination of abdomen shows a large mass the size of a baby's head, in the right lumbar region. This is movable, and the skin is not adherent. The tumor is not painful except on deep pressure. A slightly tympanic note is heard in percussion over this mass. There is marked edema posteriorly in the lumbar region on the right side, extending up along the thoracic wall. The tumor appears to be distinct from the liver. There is no jaundice. There is no pain in the lumbar region. Micturition is frequent and sometimes painful. Urine: albumin ¾ by volume. There is much pus, no casts and no blood.

She was transferred to the surgical wards for operation, but this was deemed unadvisable. Her temperature then was 101 degrees, but soon fell to normal and remained subnormal until death. Her breathing was so shallow that physical examination was impossible. Now complains of some pain in the tumor. Is having many large green liquid stools. Examination of urine just before death showed albumin, pus, casts, but no blood. She died October 26.

The autopsy was made by the writer at the Philadelphia Hospital twenty hours after death. The peritoneal cavity is normal except that the liver is pushed forward and barely

reaches to the costal margin. This is caused by a large tumor lying behind the peritoneum in the right lumbar region. The pleural sacs each contain a small amount of clear fluid. There is thickening of the pleura and adhesions at each apex. The upper lobes of both lungs show considerable consolidation at the upper portion. This is apparently due to a pneumonic process with scattered areas of caseous degeneration, especially about the bronchi. Considerable pus flows from these tubes on section and they contain caseous plugs. At the apex of the right lung is a cavity filled with purulent material. The left kidney is swollen and tense. The capsule strips readily, the cortex is swollen and of a green color, and the pyramids are congested. The pelvis and ureter are normal. The right kidney is found to constitute the tumor mentioned above. It is 25 cm. in length, 18 cm. broad and 15 cm. thick. The organ is surrounded by a considerable amount of fat that appears normal. On section the kidney is found to be extensively degenerated. The capsule is thickened and firmly adherent. The cortex measures 3 cm. in width, is dense and hard and of a chalky white color with numerous spots of extensive cheesy degeneration. The pyramids are represented by cone-shaped masses, with central cores of cheesy material which empty into the pelvis which is filled with pus and caseous debris. The wall of the cones are dense and white; the walls of the pelvis are extensively infiltrated and its mucous membrane is ulcerated.

Microscopic examination of the right kidney showed hardly a trace of kidney substance. The capsule was greatly thickened and there was an increase of connective tissue in the walls of the masses that represented the pyramids. Around the margin of the degenerated area were collections of round and epitheloidal cells and occasionally giant cells. A number of sections were stained for the tubercle bacillus but without success. However, the diagnosis of tuberculosis was made upon the characteristic picture presented by the collection of cells, associated with the extensive degeneration.

The wall of the ureter is also much thickened. This infiltration extends downward along the whole length of the tube and involves the bladder wall around the orifice of the ureter. The epithelium from the pelvis to the bladder is degenerated and replaced by caseous material, but the lumen does not seem to be obstructed. The epithelium of the bladder has apparently entirely escaped the inflammatory process.

The internal genitalia are perfectly normal. The lymphoid tissue throughout the body is apparently entirely normal. Nothing else of interest is found in the body.

Section of the left kidney revealed much fatty degeneration and chronic parenchymatous nephritis.

After a study of numerous cases reported in the literature of the subject, it seems possible to make three divisions of tuberculosis of the kidney.

1. That which occurs as a part of a general eruption of tubercle, either as a manifestation of acute primary miliary tuberculosis, or as the terminal stage of tuberculosis elsewhere in the body. This condition can not be considered to be more uncommon in the kidneys than in the other abdominal viscera.

2. In the so-called primary renal tuberculosis, the process either manifests itself in the kidney alone or this organ is undoubtedly the primary focus. This form may be either of the miliary or of the chronic caseous varieties.

3. A form in which the priority of the process in the kidney must be doubted, but in which the disease manifests itself more intensely here than elsewhere. It is associated with foci of tuberculosis in other parts of the body, apparently most often in the bronchial glands, bones or lungs. Under this division must be classed most of the cases of urogenital tuberculosis that have furnished so much discussion concerning their pathogenesis. The difficulty of accurately determining the primary seat of the disease, even in those cases formerly classed as primary in the kidney, is much increased by the possibility of there being some obscure focus of the disease in the body that escapes any but the most careful observation at the postmortem examination—a fact whose importance has been unrecognized until the past decade. Tuberculosis may exist, unsuspected, in the various lymphoid tissues, as the bronchial glands or as lately shown by Dieulafoy and Krückmann in the tonsils.

The authorities on the subjects unite in the statement that the process when once started in the kidney is extremely rapid, and it could only be by most rigid clinical study that it could be determined whether, for instance, pulmonary or renal symptoms were the first to occur.

Senator, in Nothnagel's system, who may be considered to represent the most recent ideas on the subject, doubts whether a strictly primary renal tuberculosis ever exists; but would put all cases under the third division that I have made. He is supported in this view by Steinthal.

Age and sex appear to be predisposing factors in the causation of the disease.

All authorities state that the condition is much more common in adult life than in childhood. Carr found but two instances in 120 autopsies upon children; 16 per cent. of Facklam's cases occurred under 20 years. The disease is very rare in infancy.

Males are affected about twice as often as females as shown by the statistics of Ebstein and Roberts. Osler, Rosentein, Senator and Birsch-Hirschfeld also state that the disease is more frequent in the male sex. However in Facklam's series of 106 cases there were 79 females. Hamill's list gives three times as many boys as girls.

The condition almost invariably commences as a one-sided affection, and in about one-half of the cases the other kidney is involved before death. This furnishes an argument for early operation. The disease does not appear to show any tendency to affect one side oftener than the other. The miliary form commences in the cortex, and the chronic in the medulla. The first may lead to extensive caseous degeneration, but the chronic variety is the one that produces the most extensive necrosis. Occasionally the process commences in the pelvis but usually this escapes until the later stages. The diagnosis from examination of the urine is often difficult as with the pelvis intact the discharge of cheesy material and tubercle bacilli may be entirely prevented. The ureter is usually affected. Its walls are thickened and its epithelium degenerates with the formation of ulcers. The mucous membrane of the bladder very often escapes but its wall is thickened and there are often superficial ulcers upon the trigone. The tubercle bacillus may be found in the tissues in the earlier stages, but at the autopsy the process is commonly too far advanced.

When the kidney is alone affected, most authorities consider the process to be one of hematogenous infection. The best explanation for the presence of the bacilli in the blood stream in the purely primary form, if such exist, is that the microbes have entered through the mucous membranes of the intestinal or respiratory tract without lesion, or in other words by cryptogenic infection. But, after study of a number of reported cases, the writer is satisfied that there is often a cheesy lymph gland at the root of the lung or in the mesentery, that is the means of entrance, for the bacillus to the general circulation.

Meyer of Göttingen, in 1892, described several cases in which a kidney showing chronic tuberculosis, by serial sections, he demonstrated local tubercular foci in the urinary tubules that had not extended from above or below, and which could not have come by penetration of the wall of the tubule, as its membrana propria re-

mained intact. He explained this by the assumption that the bacilli have been secreted from the blood by the vessels of the glomeruli, found their way along the tubule in the urinary stream and lodging on its wall set up their specific inflammation. He calls this form of infection "Secretion Tuberculosis." He quotes, in support of his theory, Reisser and Orth, who have demonstrated the same process in pyemic abscess of the kidney, and have shown that bacteria and even foreign bodies, as drops of fat, can be secreted through the glomeruli. Borrell, in this experimental production of renal tuberculosis by the injection of the bacilli into the aorta, has demonstrated their presence in the walls of the vessels of the glomeruli, unaccompanied by inflammatory change. Thus there is a second way in which the bacillus in the blood may produce renal tuberculosis.

Time is not given for the consideration of urogenital tuberculosis though the review of the subject does not seem complete without some mention of this form, and the discussion which its pathogenesis has occasioned.

In this variety the kidney, ureters, prostate and testicle in the male, and the tubes and ovaries in the female, are oftenest affected, much more rarely the bladder. This form of the disease is, as is natural, much more common in the male, and indeed is very rare in females.

Rayer, as early as 1837, showed that a healthy bladder may be an intermediate between a tubercular testis or prostate, and a similar condition of the kidney. The experiments of Alberran, in which by ligation of one ureter and injection of tubercle bacilli into it above the point of ligation, he proved that the disease may ascend along this tract, is conclusive, but is significant as being the only instance, in many experiments by different observers, which was successful. The ascending form is favored by Rokitansky, Lancereaux, Klebs, Steinthal, Guyon, Duret, and Virchow. Later writers, while acknowledging the possibility of an ascending form, incline to the greater frequency of the descending. So Ziegler, Birsch-Hirschfeld, Senator, Carmargo, Dickinson, Roberts, Casper, Hamill and Israel are more or less positive in the opinion that while tuberculosis may ascend the ureter, it is oftenest primary in the kidney.

The case reported belongs to the chronic variety of renal tuberculosis. The sequence of events is doubtful, and the case might reasonably be regarded as one of the rare instances of renal tuberculosis secondary to cryptogenic infection through the mucous membrane of the intestinal or respiratory tract. However, the adhesions at both apices, and the cavity on the right side, together with the fact that the lesions of the lung is of the type of an infection by inhalation, suggests that, probably the disease was primary in the lung, and that the older lesion was masked by a sudden extension of the process about it, at a comparatively late period, and shortly before death.

The histologic portion of the above report is from the Pepper Laboratory of Clinical Medicine.

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The Woes of India. The *N. Y. Sun* correspondent writing from Jhansi, India, June 22, concludes thus: The close of the sixtieth year of the Queen's reign will be remembered in India for its long death roll, its woes, and its disasters.

IS MALARIA A WATER-BORNE DISEASE?

Presented to the Section on Practice of Medicine at the Forty-eighth Annual Meeting of the American Medical Association held at Philadelphia, June 1-4, 1897.

BY JOHN M. BATTEN, M.D.
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Malaria may, to some extent, be a water-borne disease, but that the malaria plasmodium or poison gains admittance into the system wholly by way of the stomach, we think we have ample evidence to lead us to doubt. The plasmodium malariæ or malarial poison certainly gains admittance into the system through the respiratory organs as well.

I will give a hasty review of the disease as it came under my observation in 1864 and 1865. The *Valley City*, to which I was attached, had a roving permission to cruise in the sounds of North Carolina and up the rivers that empty into these sounds. The water of the sounds at times is brackish, as it has connection with the ocean through Ocracoke Inlet. The intensity of the disease and the number of cases that occurred increased in the following localities in the order named: 1, the region of the Pamlico River, upon which Washington, N. C., is situated, is rather free from malaria, though cases occur there; 2, the region of Chowan River, Blackwater River, Meherrin River, and 3, the regions of Roanoke River and Neuse River. Again, on Roanoke Island there is no vegetation, except in the northern part of it, where there are a few scrub oaks. The island is sandy and barren. Yet, one day on that island, with Dr. Walton of the 103d Pennsylvania Regiment I saw a hundred cases of malaria, but many of these cases may have been transferred from other localities. The water used there for drinking and cooking purposes was boiled water taken from the wells on the island.

The water for drinking and cooking purposes on the *Valley City* was either boiled or distilled, taken from the boiler, cooled and kept in a covered vessel for that purpose. Now, when the *Valley City* was for a long time up the Roanoke River or other rivers, the cases increased in number and intensity, according to the locality in which she cruised. When cruising up the Roanoke River for a long time the treatment of the disease in many instances would be of no avail, but afterward, when cruising in the sounds or the Atlantic Ocean the disease would respond to treatment, so that there would not be a single case of malaria on board. Afterward, cruising up any of the aforesaid rivers the number of cases and the intensity would increase according to the locality. In the region of the Roanoke River it was intensely malarious, as forest swamps and fogs prevailed there, and many places in this region were impenetrable to man or even the wild beasts of that locality. Why was there a difference in the type of the disease and also the number attacked while in these different localities? It was not the water we drank nor the food we ate, for the water we drank and used for cooking purposes was boiled or distilled. The boiler received its water by pumping from the river, sounds or ocean. Therefore the malarial poison must have been taken into the system through the respiratory organs.

In the fall of 1865, at Cairo, Ill., following a very hot and dry summer, the months of September and October were exceptionally dry and hot. I reported for duty on the United States monitor *Oneota*, lying off Cairo on the Ohio River, Oct. 6, 1865. Malarial fever was prevalent everywhere in that vicinity. A

large barge moored to the *Oneota* was filled with cases of malarial fever of a pernicious type. We used water for drinking and cooking purposes that was boiled or distilled as on the *Valley City*. What I want to show particularly is, not only that malarial fever prevailed notwithstanding that the water for drinking and cooking purposes was boiled, but that it is not necessary to have a wet season follow a dry and warm summer to produce malarial fever or the plasmodium malariæ. Up to Oct. 6, 1865, there had been no rain. The Ohio and Mississippi Rivers were very low, and along the banks of the Ohio in the vicinity of Cairo there was a green scum on the surface of the river, extending some distance from the shore. Yet, as I have said, there was malarial fever prevailing everywhere in that locality.

Again, in the Western States and in Pennsylvania malarial fever has been actually plowed out of existence. The agriculturist who, for the first time, turned up the soil, suffered the most from malaria, but as the soil was more and more stirred up the less severe and the less frequent would be the attacks, so that today in many localities where the disease was most prevalent, there are no cases of malaria. Was it the water that these early settlers drank, the food they ate, or was it the air they breathed by which the plasmodium malariæ or malarial parasites were conveyed into their system?

I hope I have made myself plain on this subject. It is only by observation and experience that we can arrive at a true understanding in regard to this matter. The more plasmodium malariæ that is taken into the system, the oftener the paroxysms of fever will recur, as every day, every second day, etc. Pernicious fever occurs in localities in which the plasmodium malariæ or malarial poison is most intense and most concentrated.

CONVERGENT STRABISMUS.

A CLINICAL LECTURE DELIVERED IN RUSH MEDICAL COLLEGE.

BY ALFRED HINDE, M.D.
CHICAGO, ILL.

I. ITS ETIOLOGY.

We have read that a child's education ought to be started before its birth; and, in our opinion, the consideration of strabismus is incomplete unless we remember that the ancestral history of these cases often contains an account of one or more persons where the visual line of one eye deviated from the object looked at with the other eye. Such predisposition undoubtedly tends to the production of squint in the offspring. The young of all animals remain with closed eyes for a variable period after birth. The human offspring is no exception. Whether this is due to a sudden change from darkness to light, or because the retinal functional development is inadequate for useful purposes, or because the brain itself is dormant, or because of all these several conditions combined, is unknown. The baby automaton has not yet awoke to the necessities of vision. Its functions are of the lowest forms of life. Its brain, as such, is yet a blank. The first promises of intellectuality are evident when it opens its eyes about the eleventh day (Petersen) and peers around. For some time afterward its vision is general and not special. Its rolls its eyes around regardless of any single object and its attention can not be arrested for a moment by ordinary objects. The center in the brain is then awakening and its anterior prolongation, the optic nerve and retina, is becoming aroused to active life, but as yet the perceptive visual centers of the brain are not alive to the requirements of binocular vision. The eyes roll according to the uncontrolled nerve explosions sent to the external ocular muscles. The complicated government of visual center over motor center is not yet completed, and will not be until the higher center can receive and appreciate the image of an outside object formed on the retina of the observing eye, in short, until the occipital cortex has grown into a sentient perceptive center and asserted its control over the motor centers of the muscles of the eye. The greater the illumination of an object the more quickly it arrests the infant's attention; hence a bright light possesses a first charm to the steady gaze. The retina and entire visual tissue need a strong impulse to whip them into useful activity. By continued practice the function of vision is developed and at last the perceptive visual centers can value at its true worth binocular single vision, and they control the motor centers of the nerves that pass to the eye muscles and so regulate their nerve explosions that the visual line of each eye accurately meets at the object looked at. Briefly, the cerebral function of binocular vision is established "by the end of the second month (Petersen) and we will assume at the same time that the acuity of vision of each eye is alike and perfect. In such a subject there is no likelihood of strabismus and it can only occur from acquired pathologic changes in the ocular tissues. Animals' eyes have a hypermetropic refraction and the newly born human eye is no exception. It is undersized until puberty[1] is reached and even then the majority of human eyes are slightly far-sighted.

The infant's eye being unduly short in its anteroposterior axis the vision of such an eye, in a condition of rest, is imperfect according to the extent of this shortening. In order to focus parallel rays from distant objects accurately upon its retina it must needs exercise its accommodation, because its dioptric system, unaided, has an insufficient refractive power for such rays. Such an eye is adapted only for convergent rays and these are not found in nature. Therefore this hypermetropic eye must either be contented with insufficient focal power, associated with a blurry retinal image and imperfect vision of an outside object, or it elects to use its ciliary muscle to relax its zonula and lens, so that the added refractive power thereby gained is adequate for punctate union of luminous rays entering its pupil. Within a limited range this function of accommodation can be exercised alone or without any associated convergence, and it is by such means that youthful far-sighted eyes can add to their established binocular vision the power of seeing distant objects clearly. With binocular fixation, therefore, and normal acuity of vision such eyes are free from the probability of strabismus. The infant considers only distant objects and in predisposed cases strabismus does not occur until the child's curiosity has called forth the desire to observe near objects. This intellectual accomplishment is not reached earlier than the second year of life and it is during this period[2] that our squint cases begin to develop. They increase numerically up to the sixth year and are of the convergent variety.

[1] Twelve to eighteen years of age. Landolt, "Accommodation and Refraction of the Eye," p. 444.

[2] According to Petersen (Amer. Journal of Insanity, July, 1896), a normal child begins to distinguish colors correctly about the age of two years.

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We see that it is not the distant object that brings out the strabismus, but the nearer object. We have averred that parallelism of the visual lines was consonant with distant vision. The two visual lines were accurately fixed upon the far away object, we had no strabismus.

The near object has called forth a new function, viz., that of convergence of the eyeballs. The function of accommodation and convergence, save within narrow limits, act together. The eyes we have mentioned as seeing accurately at a distance without either of them deviating were somewhat hypermetropic, but the license existing between accommodation and convergence was enough to permit of sufficient exercise of the former for distinct vision, without calling forth the latter faculty. This amount of hypermetropia has been called *facultative* (Donders), and in amount, in our example, is insufficient for the development of squint.

A higher degree of far-sight can not thus be neutralized. The link of union between accommodation and convergence is now put upon the stretch and accurate vision, *i.e.*, punctate focusing of rays upon the retina, can only be done by a greater refractive power of the observing eyes, while this can alone be accomplished by still greater contractions of the muscles of accommodation, and this stronger nervous impulse can not be exercised singly.

In the posterior part of the floor of the third ventricle of the brain and in the floor of the aqueduct between the third and fourth ventricles are several groups of nerve cells that give rise to separate groups of nerve fibers. The nerve fibers are gathered together into the third nerve trunk, but these separate groups of nerve cells give off nerve fibers that have separate and distinct distribution to the motor apparatus of the eyeball. Three of these central nuclei lie side by side and undoubtedly have intercommunicating fibers for the functions of the distal structures of distribution, save within narrow limitations, are always exercised together. I refer to the nuclei giving origin to fibers going to the ciliary muscle, the most anterior central group of cells; to the sphincter of the iris, the group equally forward and to the outer side of the ciliary muscle group; and to the internal rectus muscle, the group of cells in the median line immediately behind the ciliary muscle group. Neither does the proximity of origin of the these fibers terminate the close relationship of function, because they travel side by side in the third nerve trunk and only separate where its lower division within the orbit gives off the short motor root of the lenticular ganglion. From this point the motor fibers to each internus travel alone and the fibers to the intra-ocular muscles after filtering through the ophthalmic ganglion travel together in the short ciliary nerves to within the eyeball and are supplied from the same plexus within the ciliary body, though they are not completely inseparable, as shown by numerous cases of separate paralysis of either the sphincter of the pupil or of the accommodative muscle alone.

Thus we see the close anatomic relationship of the functions of accommodation and convergence and find at the same time the reason for their almost[3] simultaneous activity.

It is undoubtedly a fact that each of these nerve groups can transmit an independent amount of nerve force, but this is of limited degree and the function of the three centers is practically an associated one.

Now we will suppose that our child, who is beginning to observe near objects, possesses a medium amount of hypermetropia (2 to 6 D.) and has established the faculty of binocular vision for distant objects. He may apparently see objects in the distance distinctly, for he does not deviate either eye from its proper line of direction. When he looks at the near object, however, the inaccuracy of his vision is of such a high degree for these divergent rays that he must needs contract to a still greater extent his accommodative muscles, and in doing so he oversteps the bounds of independent action of the ciliary groups of cells over those of the interni, and this excessive effort at focusing becomes associated with an excessive effort of convergence, with the resulting deviation of one eye; a sacrifice of binocular vision, but gaining a more accurate focusing of the object with the other eye alone. In short, a strabismus convergens is instituted, and seeing that it occurs only when looking at a near object it is called a *periodic strabismus*. At first this squint may not be constant for near objects, if the latter are large enough and well illumined. Its frequency of occurrence, however, usually increases until for near it becomes permanent and soon it occasionally appears for distance, while later it becomes a fixed condition for objects at all distances.

It may be observed that the squint changes from one eye to the other, either eye fixing the object. This variety is known as *strabismus alternans* and the vision of each eye is usually equally good. In the majority of cases however the eye that first deviates inward is the eye that continues to squint and sees less well. This is known as *strabismus concomitans*, because the squinting eye follows its fellow and the angle of deviation is always the same. In this latter variety of squint the vision of the two eyes may at first be equal, and in this as well as in other forms of strabismus when vision is present in the two eyes, a diplopia occurs, because the two lines drawn from the maculæ through the nodal points of the two eyes do not meet on one and the same object. The eye whose visual line is properly directed has upon its most sensitive retinal area, the macula, an image of the object looked at, more distinctly perceived than the image of the same object that falls to the inner side of the macular region of the deviating eye, upon a portion of the retina with fewer cones and hence less sensitive in perceptive elements. Thus you see that the images cast upon the two retinæ do not fall on corresponding areas, hence double vision exists and the nearer these images are together, with retinæ equally percipient, the less the difference in distinctness and the greater the annoyance this diplopia causes the patient. But strabismic children, if they realize its presence, do not complain of or mention this double vision.

If there exists in our little patient a still higher degree of hypermetropia (above 6 D.), together with binocular vision, the vision for distance may be so poor and the knowledge of the value of more perfect vision so little appreciated, and the efforts of accommodation so unavailing or unused, that the child appears to rest contentedly with the vision that exists, together with a condition of relaxation of its efforts of accommodation and convergence. In short, it retains its binocular fixation and is satisfied with its poor vision until the prolonged efforts of an insufficient accommodative power result in subjective

[3] In point of time the contraction of the pupil occurs later. (Donders.)

symptoms of eye distress and headache, associated with positive inability to see small near objects in school work. The oculist is then appealed to, not because of the deformity of strabismus, for this is absent, but because of the eye distress and the insufficiency of useful vision. Convex lenses are prescribed that are an accurate measure of the deficiency and when worn the symptoms cease and the required studious work goes on satisfactorily, though usually with subnormal vision, because these eyes are not only illy formed, but also histologically imperfect.

In the foregoing illustrations we have seen that in the hypermetrope of low degree the accommodation was sufficient for perfect vision, without calling upon the aid of convergence, and binocular vision was maintained, with no strabismus resulting.

In our hypermetrope of medium amount the accommodative effort was so extreme for accurate near vision that it could not be exercised without calling forth an effort of convergence in excess of the requirements of binocular vision. Briefly, a deviation inward of the visual line of one eye had occurred, a strabismus was instituted and binocular vision was sacrificed upon the altar of more accurate focusing.

In the highest degrees of hypermetropia we have shown that binocular fixation is retained and accurate vision correspondingly suffers, although squint does not always occur. The preceding illustrations are the rule, but not the invariable rule.

In all, hypermetropia was present, and this error of refraction occurs in three-fourths of all the cases of convergent strabismus. The cause of the remaining one-fourth of the cases of converging squint remains to be explained. An eye with a normal refractive power, an eye that in a condition of rest of its accommodation, has the power of punctate focusing of parallel rays upon its retina; an eye whose dioptric system unaided has sufficient refractive power for perfect distant vision, an emmetropic eye, may be the subject of strabismus. Binocular vision may have been inaugurated but inevitably lost because of a loss of balance of the lateral eye muscles (Schweigger), a congenital weakness of the externi or a superabundant power of the interni, either congenital or acquired. This condition explains the cause of a majority of the remaining cases of inward squint, and the above muscular conditions combined have a very important bearing in the causation of the ordinary cases of hypermetropic squint, as we shall see when we consider the operative measures necessary for their cure.[4]

Again, a muscular weakness in the interni, usually congenital, but occasionally acquired from the exhaustion of overwork, even with a hypermetropic refraction, may permit of a divergent squint. The opposite is occasionally true with a myopic refraction, a refractive power that is relatively too great, an anteroposterior axis of the eyeball that is excessive, so that parallel rays entering the eye are brought to a focus in front of the retina, we may have a convergent strabismus, and this is due either to a superabundant power of the interni muscles or a deficient power of the externi. The adductive power of convergent strabismic eyes is commonly too great, may be congenitally so, may be acquired from constant effort at convergence with accommodation. The shortened distance between the origin and insertion of the internus as a constant quantity results in a permanent shortening of this muscle (Hansen-Grut's theory) and also of the superior and inferior recti.

Lastly, the more anterior insertion of the internus tendon—six millimeters from the cornea—gives it a great advantage over the weaker externus, whose tendon is usually eight and occasionally ten, or even twelve millimeters from the outer limbus corneæ. Moreover, the position of rest of the entire ocular muscles is one of slight convergence of the visual lines.

Other causes of convergent squint are found in a congenital amblyopia of the squinting eye (Schweigger 30 per cent.) whose vision is too poor to exercise any efforts toward the establishment of binocular vision. It permits itself to be rolled inward by the stronger internal rectus, and so remains. Even in eyes where one has a greater error of refraction than the other, so-called, *aniso-metropia*—the one with the greater error, hence the poorer vision, has the least hold over the binocular fixation, and occasionally gives it up altogether and permits the internus to sway the eyeball inward. It is the rule in concomitant strabismus convergens for the eye with the greater error to be the deviating eye. Again in spasmodic conditions of the interni, produced by central, distal, or general causes, a convergent squint occurs, stays for a variable period, may recur, and happens generally after strabotomy, or in the constitutional conditions of children. This form is known as an *intermittent strabismus* and is infrequent.

Lastly, young patients are prone to squint where an acquired permanent opacity occurs as the result of some preceding inflammation of the cornea. Binocular vision, if instituted, is surrendered because of the inaccurate vision of the affected eye and the strongest muscle turns the eye inward or outward according to the relative strength of the individual muscles.

There are exceptions to the rule in strabismus, as elsewhere, and a few examples of this condition do not conform to the foregoing causative explanations. We occasionally see a case of squint, where only a lower degree of hypermetropia is present; where distant vision is accurate; and where the only deficiency we can find in the case is one of markedly diminished amplitude of accommodation for the age of the patient, an excessive convergence is instituted to increase an insufficient accommodation.

One of these cases I have lately had under my care where a patient, age 16, had an alternating convergent strabismus of 60 degrees with 20-20 vision in either eye without glasses, and who developed under atropin-paralysis only a total hypermetropia of 1 D. with the right eye, and 1.50 D. with tho left, and who, after the atropin effect had entirely passed off, read with these glasses, either eye alone, 0.60 type of Snellen at a near point of eight and three-fourths inches (22 centime-

[4] The last case of convergent squint I showed you was due to a combination of hypermetropic astigmatism, amblyopia and a lack of balance of the lateral muscles of the squinting eye. The patient, a girl of 18 years, had had a tenotomy of the left internus when 12 years of age, but objected to the use of glasses afterward. She appeared with a still present but very variable convergent squint. A parallelism for the distance but at 5 inches now, a deviation inwards of 15 degrees, and again 30 degrees, and still again an associated movement of the squinting eye outward, a divergence in fact being present. Yet in extreme abduction the left or squinting eye failed to roll out nearly as far as the other eye, even though a tenotomy of the opposing internus had been done six years ago. Here was present a marked weakness of the left externus muscle. The vision of the straight right eye was 20-40—; that of the left 20-200, while under atropin-paralysis that of the right eye was 20-200 and of the left eye 8-200. With each eye + 3.50 D S ⊂ + 1 D cyl., axis 90 degrees, the right eye had 20-30 and the left 20-200. Therefore the best vision we could get with the straight eye was only two-thirds normal, and with the deviating left eye only one-tenth normal, yet the refractive condition of the two eyes was the same. The squinting eye was very amblyopic and besides had an anomalous muscular condition causing an ever variable direction of the eye, and the principal annoyance of which the patient complained was a constant diplopia. Further operative procedures were inadvisable and the above glasses were given to be worn constantly.

ters), showing an amplitude of accommodation of only 4.5 D. whereas it ought to have been about 11.5 D. at that age. We had a deficiency of 7 D., in short, the accommodation of a person of 40 years old. An advancement of each externus, with the requisite glasses for distance and near, resulted in a complete removal of the deformity. Another instance, where, with a high degree of hypermetropia, we have deviation inward, an example of this kind I have recently shown you in a hypermetrope aged 7 years with a Ht each eye of 7 D. Again, where, with a difference in the amount of the error of refraction in the two eyes, the one which has the lesser degree is the strabismic eye. The child recently before the class furnishes another, or central etiologic cause. She fell several feet one and a half years ago, and injured the occiput. This was followed by intra-cranial symptoms and intense headache, and soon afterward a concomitant strabismus inward and upward of the right eyeball, developed and has remained. The lateral deviation measured five millimeters and the ophthalmoscope showed a secondary optic atrophy most advanced in the right eye. As an observation, the retinoscopic test gave a total hypermetropia (under atropin) of the right eye, of 2.50 D., whereas in that of the left, straight eye, the amount was 3.50 D. Whether this error of refraction had any bearing on the production of the squint is a question. The more probable cause is the poorer vision of the right eye permitting the suspension of binocular fixation, together with the excessive action of the right externus muscle. The child is in her eighth year and was taken from school because of her failing eyesight.

One other case, is of retinal pathologic causation, and, in point of time, the lesion that was the immediate cause of the strabismus convergens was congenital, yet the squint did not show itself until the second year of life, when the continued functions of accommodation and convergence were established for near objects. The cause of the squint in the left eye is due to a macular destruction of the retina from a subjacent choroidal atrophy. The latter of small area, sharply defined, single and due to a prenatal circumscribed choroiditis. This boy, now about 8 years old, always closes the squinting eye when looking at near objects, because of some interference with accurate vision of the other eye, and though he can not explain it, it is most probably due to the recognition of the false image of the misdirected eye, a diplopia. The refraction of the eyes is emmetropic; therefore, the only explanation of the squint is that of the macular lesion of the affected eye, and the latter permits itself to be dragged inward by the stronger internus muscle.

There are also other exceptional causes of strabismus but all of these together do not invalidate the foregoing teaching. They only place us on our guard in our investigations; perfect our methods; and make of us more accurate observers. A consideration of the etiology of strabismus convergens would be incomplete without paying homage to that master-mind of Donders, of Utrecht, who thirty-three years ago, in his immortal work: "Accommodation and Refraction of the Eye," gave to the medical world the first clear conception of the causation of convergent squint; stated that 77 per cent. of the cases were due to hypermetropia; and placed the proper value on the use of convex lenses for the correction of the anomaly.

II. Its Management.

When considering the etiology of strabismus convergens we concluded that there were three quantitative degrees of hypermetropia that especially required our attention, and that the cases of medium amount (2–6 D.) of this anomaly furnished three-fourths of our cases of squint. The possessors of a low degree (up to 2 D.) of far sight, are often not seen in practice until the middle period of life, when near work becomes tedious and subjective eye symptoms force the patient to wear glasses some years earlier than the usual presbyopic time.

Those children who have the highest amount of hypermetropia (over 6 D.), are dismissed from school because they can not see the markings on the blackboard and appear mentally dull. They are advised to see an oculist, glasses are fitted and they return to their studies. What are we going to do with our young hypermetropes who squint? Estimate the amount of their error under atropin and give fully correcting glasses, to be worn constantly during the waking hours, together with the use, for a time, of atropin daily to annul accommodative efforts and break off the formed habit of over-convergence previously needful for accurate vision, especially of near objects. If we can demonstrate an amblyopia of the deviating eye, supposedly due to non-use, we stop using the atropin first in this eye, and advise[5] the practicing of its vision alone for a time, thus giving this eye an advantage for near use. Later stop using atropin in the good eye and still practice with the weaker eye, twice daily for an hour or more, the good eye being covered at the same time.

The amount of error is estimated usually by means of retinoscopy (skiascopy), this plan being more accurate than the ophthalmoscopic measurement, and even where our patient is able to read type, the subjective test is less reliable than by the first objective method mentioned.

The glasses given are changed as needed and are worn faithfully, until we are assured that they will not alone avail for the cure of the deviation, and until the patient is old enough for the operative procedure. In cases of supposedly spasmodic squint it is well to defer operative procedures indefinitely, also in cases of alternating strabismus, and in cases where repeated measurements give variable amounts of deviation, be slow to operate.

In squint cases where the error of refraction, on re-examination, is decreasing, and in families where strabismus has been known to continuously disappear, do not operate until after puberty or full growth of the eyeball. The correcting spectacles, without or with operation, must constantly be worn to avoid recurrence due to the still present hypermetropia. Now we have remaining the cases of squint where intracranial or retinal causes produce the deviation indirectly by the resultant poor vision. If operation is decided upon in these cases it is solely for the cosmetic result. Here we can not expect to institute what has never before been established, viz., binocular single vision. In fact, in those cases that we have still left as suitable for operation, we can not expect to regain this great desideratum—the aim of all of our surgical procedures—in more than 10 to 20 per cent. We may get parallelism in 50 per cent., and we expect satisfaction

[5] Blindfolding the sound eye and practicing the deviating eye was ninety years ago one of the successful methods of correcting strabismus, and Mackenzie quotes (Amer. Ed., 1885, p. 367) the case of Professor Roux, who thus cured strabismus in himself of thirty-five years' standing. Beer (loc. cit.) was in most cases successful "by binding up the sound eye every day even for a couple of hours only."

in 80 per cent., or, by our present methods, a success in practically all of our selected cases. That the subject of the operative management of strabismus is still unsettled in its entirety, is evident from the continued discussion of the subject in the journals and in the special medical gatherings throughout the world. Even as lately as the meeting of the Ninth International Medical Congress (Washington, D. C., 1887), several members admitted that the correction of strabismus was not a satisfactory operation.

The first operation on an ocular muscle was done by Stromeyer in 1838 on the cadaver (Mackenzie, p. 369, Amer. Ed. 1855). "Pauli was the first to attempt it on the living, but the eye was unsteady; he divided the conjunctiva but could not divide the muscle" (Mackenzie, pp. 369, 370).

Later, in 1839, Dieffenbach operated on a living patient, cutting through the belly of the muscle. The posterior half of the muscle then retracted, so that no reattachment to the eyeball could take place. The complete paralysis of the cut muscle was followed by a divergent squint. Such result was observed to be invariable after this method of operating, and the procedure fell into great disrepute (Fuchs, p. 710). Böhm then suggested a section of the tendon of the deviating muscle, and such method has been continued ever since.

Von Graafe improved the technique of the operation, showed how to increase and diminish the effect, and instead of cutting the tendon, when in the grasp of forceps, he introduced his hooks and the section was made on these. Both of the latter methods are still in use. The result from the Dieffenbach operation was so disastrous that the subjects of it presented material for further corrective procedures, and Jules Guerin, in 1849, operated on these cases of divergent squint with the view of advancing the attachment of the cut internus toward the cornea. He dissected from the inner portion of the sclerotic all the superjacent structures—conjunctiva, Tenon's capsule, internus, and far back toward the equator of the eye. He then placed a thread *through* the sclerotic on the external side of the cornea, and by drawing on both ends of the thread he rotated the eyeball inward to the desired extent, and fastened the thread ends, by strips of plaster, to the skin of the nose. In this way it was hoped that a more anterior attachment of the severed internus would take place. The thread was left *in situ* several days, but it was observed to cause serious inflammation in some cases, now known to be due to a traumatic cyclitis, because of the depth of insertion and the location of the thread.

Now the ingenious von Graafe came to the rescue, and proposed the so-called *thread operation*, which differed from Guerin's plan only in that the internus was not severed from the sclerotic immediately at its insertion, but a short distance behind it, so that a stump of the tendon was still adherent to the sclera. Through this stump the thread was passed and the eyeball guyed in and fixed, as before, in the desired position.

Since then thread methods of advancement have been greatly improved, and several plans have been followed—some consisting of three threads, some of two threads, some where a single thread is advocated. They all agree in attaching the thread or threads directly to the severed tendon of the muscle to be advanced, and stitching the forward end or ends to the eyeball in front, where traction on the thread draws forward the muscle and tilts inward the eyeball to the requisite extent, and so retains the two fixed points for the necessary days required for the cicatrization of the wound at the site of the tendon of the advanced muscle.

From this history you will see that we have established two procedures, one of tenotomy of the insertion of the tendon of the internus in cases of convergent strabismus; the other of advancement of the retracted muscle subsequently to a former tenotomy of it, in order to correct the opposite deformity, a divergent strabismus. These operations on the internus tendon are equally applicable to that of the externus, though rarer; also to those of the other straight muscles of the eye—the superior and inferior recti—though operations on these tendons are still less frequently required.

We are considering a convergent squint and its correction, and we want to lessen the over-effect of this internus muscle on the deviated eyeball. As we have seen, when considering the etiology of squint, that this muscle usually possesses a superabundant strength, when compared with the often congenitally weak externus, and that owing to a continuation of the deviation a permanent shortening of the distance between the origin and insertion has occurred. Besides, its anterior attachment to the sclera is always further in front of the center of rotation of the eyeball than that of the less favored externus.

The indication is then plain; we must increase the distance between the origin and insertion of the internus muscle, or weaken its strength, or both, in order to destroy its over-effect on the deviated eyeball.

How are we going to do this? 1. By displacing backward the anterior attachment of its tendon to the required extent. In cases where the deviation does not exceed three millimeters, a careful section of the tendon of the internus at its insertion is usually sufficient to correct it. In cases of four to five millimeters, if tenotomy of the shortened internus insufficiently corrects, the teaching has been to free the lateral expansions of the tendons that are derived from the capsule of Tenon, where the muscle passes through the latter, near the equator of the eyeball. These connective tissue fibers on each side of the tendon are cut to the requisite extent. If the eye then fails to roll outward sufficiently, a guy suture is inserted vertically through the conjunctiva, just beyond the outer margin of the cornea, and one end passed through the outer commissure of the eyelids. The two ends are then tied, with traction sufficient to drag the eye outward to a correct position. This traction results in a displacement backward of the cut tendon, and the thread is left in from a few hours to two days.

Where the strabismus measures five to six millimeters, the cutting of one tendon is insufficient for correction without displacing backward the muscle so far that the resulting loss of motion inward impairs the future usefulness of the eye, when observing objects situated in the extreme nasal field of this eye. The operated eye then fails to follow the other eye, and binocular fixation does not occur. Hence, the operation effect has to be divided between the two eyes. The deviating eye is tenotomized in its internal rectus, and after waiting a couple of weeks the remaining squint is removed by a sufficient operation on the internus tendon of the other eye. Thus the inevitable loss of motion that follows all successful tenot-

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omies is divided between the two eyes. Where the strabismic deviation reaches eight millimeters the last directions are carried out, except that the lateral expansions must also be invaded with the scissors, in order to get a sufficient result. When the deviation is over eight millimeters, the teaching has been, and still is in many quarters, to make a simultaneous and very free tenotomy on each internus, and if the effect, after healing, is still insufficient, to again cut the internus of the eye that retains the best motion inward. Sometimes this extreme procedure proves insufficient and both muscles have been tenotomized a second time, and such eyes may then possess a parallel position for distance, but too often their power of convergence is so reduced, that for near use they are a source of great trouble and anxiety to their possessor, if indeed they do not later give up the fight for binocular single vision, one eye making an associated movement while the other fixes—briefly, a divergent squint occurring as the final result of over-operation. The latter effect may occur at any time from one to two years after operation, and though often seen after excessive operative procedures, yet it occurs very frequently after ordinary and legitimate tenotomies. Unfortunately, we have no positive means of foretelling its occurrence in the latter cases. Tenotomy of the lateral ocular muscles is a very simple operation, so far as finding and cutting the tendons is concerned. Very little anatomic knowledge, steady hands, a forceps, scissors and, if you wish, a pair of hooks, a blepharostat and a solution of cocain, are all that are required. We can not foretell positively in any case the results we will accomplish by such cutting.

The first internus tendon I ever cut was in a boy of 14, and without the slightest immediate change in the direction of the eye. I inserted into the wound made the smaller von Graafe hook, and made excursions clear back to the equator of the eyeball, and laterally in both directions, thus breaking down and cutting all the loops of tendon and connective tissue brought up on the hook, but still the deviation remained unchanged. I was nonplused, and felt confused, for others were present, so I stopped, knowing that there were matters in squint that were either not in the text-books, or I had failed to read them. My case was not a paralytic squint. The boy disappeared, without a correction, and I went to further observation and soon saw the same thing occur in the practice of the best men, two of whom had one case each where, after a tenotomy of each internus, there was no effect, and one put on guy threads and the other stopped operating and said nothing. A second head of insertion uncut and far back is not a sufficient explanation in these cases, for often we can not find it after prolonged search. Paralysis of the externus is excluded, as the eyeball is rolled directly outward. A shortening of the superior and inferior recti muscles is the common cause that keeps the eye from rolling outward. For the latter reason, the backward displacement of the cut internus is also limited, and besides a tenotomy of it, we need an opposing force to the other shortened and uncut structures. We have mentioned a guy thread, but this cuts out and is of only temporary use, while it is a bungling method of correction.

2. When considering etiology, it was seen that the more posterior attachment of the externus tendon, together with the relative weakness of this muscle, placed it at a great disadvantage; besides, when the distance between its origin and insertion is increased by the deviation of the eye inward, in convergent squint, it can readily be seen that, next to the cutting of the shortened tendon of the internus, a strengthening of the effect of the externus on the eyeball ought to be accomplished. As a matter of physics let us shorten the distance between the origin and insertion of the externus by cutting its tendon at the sclera, removing two to four millimeters of it, and reattaching the remaining tendon as far forward on the sclera toward the cornea, as the requirements of the case demand.

These two operations—a careful tenotomy of the shortened internus, together with a simultaneous shortening and advancement of the externus—have scored a great advance in the more certainly beneficial results of strabismic operations.

This combined operation was first advocated only in cases of divergent squint, where a tenotomy of the externus alone never exceeds a correction of over three millimeters, and where such section is wholly insufficient for a satisfactory correction. Of late years, more especially under the able advocacy of Landolt, this procedure has been applied to convergent squint, and has grown in favor so rapidly that at present it promises to soon be the only procedure for the correction of squints of over three millimeters in extent. The results are positive and can be more accurately calculated, but in the cases where it is most used it is found necessary, for a thorough correction, to advance the externus of both eyes—not simultaneously, but on the second eye after an interval of two weeks or more.

The objections to tenotomy are its uncertainty of results, the opposite deformity occasionally occurring, and the loss of motion invariably following the successful correction of the deviation. These conditions are absent after advancement; in fact, the eyeball retains its position in the orbit, is directed permanently correct, and its motility is greatly improved.

Most modern ophthalmologists consider that tenotomy ought to be limited to cases of motor insufficiency, latent squint, and to the slighter actual deviations, but Landolt believes that in all these cases advancement offers greater advantages, and he claims that in his hands it has never caused an over-correction.

There are advocates of an advancement of the antagonistic muscle alone; the externus in convergent, and the internus in divergent squint, without a simultaneous tenotomy of the deviating muscle. There are others who believe in a partial tenotomy of the shortened tendon, leaving a few strands of the tendon proper at its upper and lower border, associated with an advancement of the antagonistic muscle, desiring to only temporarily weaken the deviating muscle until the advanced muscle gains a firm reattachment (Beard). In the two latter procedures an increase in motility of the operated eyeballs is very marked, but the certainties of full correction are less assured.

Modifications of the foregoing procedures, in different cases, are carried out according to the dictates of the various advocates, and no consensus of opinion has yet limited different methods to special conditions. One certainty is accomplished because of the better results achieved, viz., that the champions of advancement are increasing greatly, and the cases where tenotomy is alone required are rapidly disappearing from our observation.

SURGERY ONE HUNDRED YEARS AGO.

AN HISTORICAL STUDY

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X.—ENGLISH SURGERY. JOHN HUNTER.

(*Continued from page 284.*)

Hunter had attained the height of his glory in the middle of the eighties. He enjoyed the confidence of the public, had a very large practice and made a sensation through a few operations. In spite of the mass of business in which he was involved, he showed himself equal to it all. In 1790 he assigned to Home, who at his request a few years previously had been substituted for him in St. George's Hospital, all lectures, because they cost him too much time, and he wished to put his writings in order. His health had also suffered a severe shock, so that he no longer went to patients at night, and he never operated without the assistance of another surgeon. He had no lack of external honors, having been made surgeon-extraordinary to the king, first surgeon general in the army, and inspector general of the military hospitals. He became a member of the Royal Society of Sciences in London and Gothenburg, of the Académie de Chirurgie and the Royal Society of Physicians in Paris, of the American Philosophical Society, of the College of Surgery in Ireland, and last of all a member of the Medico-Chirurgical Society in Edinburgh. Besides occasional attacks of the gout, during the last twenty years of his life he suffered much from angina pectoris. Often he saw everything aslant (which he declared, was caused by an excessive contraction of the oblique muscles of the eyes, which drew both eyes about thirty or forty degrees from their natural position); his memory became weaker, and the asthma grow worse. On Oct. 16, 1793, he went into St. George's Hospital become violently angry, and fell to the floor, dead. They found a defective valve, with enlargement of the aorta. Only a few of his oldest friends accompanied the body to the church of St. Martin in the Fields.

John Hunter was, as his brother-in-law relates, small and compact, but strong; his countenance animated and open, in later years contemplative. As a young man he was very lively and given to pleasures, later on account of a difficulty in his chest he could drink no wine, and for the last twenty years drank nothing but water. He needed little recreation; he slept only five hours. Passionate by nature and easily aroused, he was pacified only with difficulty. His mind was extraordinarily active and showed a natural disposition for investigation. His practice had grown but slowly at first, for his love of independence lead him rather to devote himself to his favorite pursuits, than to widen the sphere of his public activity. Only when his great talents attracted general attention did his income increase considerably and amounted in the last years of his life to five or six thousand pounds. But he valued money little, and devoted everything to collections and investigations, by which the interests of himself and family suffered. Very open hearted in private practice, he always expressed his opinion without reserve, but was at any time ready to acknowledge a mistake. An enemy to all deceit and falsehood, he, in going about, spoke almost too freely and often harshly of his colleagues, by which he made many enemies. History places little value upon such weaknesses of great men, but their contemporaries often have to suffer severely from them. If Hunter did not do justice to the merits of others, it was not due to envy nor jealousy, but to the conviction that surgery was still in its infancy and he himself only a beginner in it. Therefore, in his effort to perfect the science, he undervalued those whose activity did not equal his own.

When John Hunter, in the twentieth year of his life, went to England, where facts were valued most highly and prätical effort everywhere prevailed, he began with admirable industry to make experiments and to collect observations. This was the most important feature of his life. His investigations included the whole animal kingdom, from the smallest insect to the elephant, in the course of which he dissected more than five hundred different species, and made a great number of discoveries. He found that bees do not collect the wax but secrete it; discovered the ability of the mollusks to absorb their shells, the true nature of the circulation of crustaceans and insects, the organs of hearing of the cephalopod, the semi-circular canal of the cetacean, the lymph glands of birds and the air cells in their bones. He wrote concerning the electric organ of the torpedo, the organs of hearing in fishes, the maw of the salmon, and on wall-fish. Young animals were fed with madder, and in that way the laws of growth and bone formation were ascertained. He furnished the information that the wolf, jackal and dog, belonged originally to one species, and by the incision of an ovary made researches in regard to the number of young. In the human body, he discovered the muscular membrane of the arteries and iris; he found that a few branches of the n. olfactorius came from fifteen cranial nerves; he traced the arteries of the pregnant uterus to the placenta, described the depression of the testes in the new-born child, by means of the gubernaculum called after him, and asserted the digestion of the stomach after death, through its own gastric juice. He also showed that the red corpuscles later form other constituents of the blood. Then he turned to the plant world and wrote on the power of plants and animals to generate heat; and there followed investigations in minerals and crystals. In short, Hunter wished to unite all branches of natural science, in order to advance from the simplest to the most highly developed forms; he asserted that nature observes a regularity even in her variations, and that under certain conditions a variation is a part of the law of nature.

His physiologic principle was "vital power," which held sway, even to the time of Johann Müller. The theory of the vitality of the blood did not originate with Hunter, for Harvey had already asserted it, but Hunter sought to give it a firm foundation and his opponents at that time were not able to expose the fallacy of this theory with sufficient reason. That this theory could have been held so long, came about as it so often happens, from the fact that the ideas of great men are so very seldom examined with attention, and tested. He said, "to conceive that the blood is endowed with life while it circulates, is to make the utmost demand upon the imagination; the difficulty arises from the fact that the blood is a fluid and we have not been accustomed to think of fluids as living. Disease manifests itself more clearly in the blood than in any other part of the organism, and all this is said to proceed from a dead animal fluid, upon which

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a disease of the solid parts makes such an impression! That is to attach too much importance to the solid parts and too little to the fluid. When one reflects upon all that concerns the blood, then the idea that it is living is not so difficult to grasp, and when he has once seized it, I can not conceive how one can think any other theory possible, so soon as one considers that all the parts are built up from the blood, that we grow by means of it, and that if it does not already possess life before this operation, it must receive it during the process of formation, since the parts, when they are once formed, possess life, and of that we are all agreed. William Hunter was the first to point out the fact that a callus is endowed with life as well as the bones. The vital principle of the blood, which is similar in its action to the vital principle of the solid parts, owes its existence to the same matter from which the latter is derived, namely the materia vitæ diffusa, which is equally diffused through all the solid and fluid parts." Hunter had an aversion to definitions, and instead of giving one for life, he stated its properties, saying that the first and simplest conception of life is, that it is the principle of self-maintenance and then the principle of activity. Though Haller has the credit of having first separated the human body into its anatomic elements and specified the physiologic characteristics of each, Hunter (who has left behind him ten folio volumes of physiologic notes) endeavored to apply these studies to pathology. The connection between physiology and pathology underlay all his theories; when he adduced the laws of life and applied them to the explanation of disease, he found a new method of inquiry.

In spite of his great accomplishments in physiology, the sick man was always to him the favorite object of investigation. They were accustomed in England to proceed by the induction method, while in Scotland the conclusions proceeded from general principles to particular cases; in Scotland a great logician was considered a great man, in England only when he had proven his position by experience. Buckle said that Scotland, where Hunter spent his youth, implanted in him the deductive method, and England, where he lived for forty years, gave him the inductive method. When his mind was divided between the two methods a certain obscurity marked not only his words, but also his thoughts. That was his error. Both methods are most clearly evident in pathology. With his inclination for deduction he endeavored, among other things, to introduce the idea that all diseases develop more readily on the skin than in the internal parts, by virtue of a latent impulse, which also impelled plants to approach the surface of the earth. Another deductive proposition was the following: "Since I regard every process in the organism as an action, whether it be general or local, it seems to me without doubt, that two different actions can not take place in the same constitution, or in the same part, at the same time. From this principle it follows that two different fevers could not appear in the same constitution at the same time, nor two local diseases in the same part." With this inclination of mind, Hunter roughly opposed the traditions of the English. However, a large part of his investigations were, as far as possible, inductive. He neglected no opportunity to make a dissection, anticipating that the progress of medical science would depend on the general introduction of dissection. He made pathologic anatomy at home in England and gave it an immediately practical direction, and was, therefore, perhaps even more influential than Morgagni's works.

He especially endeavored to make the knowledge which he gained of value in the perfecting of surgery. A large part of the surgical improvements which were introduced during the next decade after his death can be traced back to him; even his expressions were for a long time common in the English schools and exercised an influence upon the style of the new surgical books. The most important among his surgical works, which we will later enter upon more particularly, is "A Treatise on the Blood, Inflammation and Gunshot Wounds (1794). In this work, which, like all products of genius, exercised an inexpressible charm, there was, properly speaking, a new method of investigation in surgery laid down. The treatise on inflammation, which was first outlined in 1762 after the retaking of Belle Isle, and arranged from observations which Hunter had made during twelve years in London, was from that time forward considered by the public as his chief work. In the course of these investigations he frequently allowed himself digressions which have a remote connection with the main subject, while he perplexes the reader by his lack of form. But a part of his obscurity is explained by the fact, that at that time he was often sick and depressed, and he died while engaged upon the complete correction of the work. In order to offer as complete a book as possible, he had long delayed the publication of it, and hoped that it would enable others to work upon a subject of which they had previously known nothing at all. He, himself, regarded this treatise as a new building put together out of crude materials, whose defects were not unknown to him. Further, we owe to Hunter the first knowledge of phlebitis (1793), investigations in varices, aneurysms and with this the ligature of the arteries above the sac (1785), the forerunners of subcutaneous operations, clear-sighted treatises on syphilis (1786), the "Structure and Disease of the Teeth" (1771), intussusception, anuresis, etc. He was the only one who comprehended the first ideas of vaccination of his pupil Jenner, and mentioned them in his lectures as a matter that deserved further research. When his operations failed, he sought for the causes with the utmost care, and in this way discovered many imperfections. The successful extirpation of exceedingly large tumors in the neck testifies to his operative skill; in one case, one of the best surgeons in England had declared that only a fool or a lunatic would venture to cut out the tumor. His skill in diagnosis, for that time very great, was shown, among other ways, in a case of a gunshot wound in the abdomen, where the others ascribed the tension of the abdomen to the flow of blood, while he convinced himself by "percussion," that air was the cause of it (1783). He also diagnosed an organic defect of the heart and found, on dissection, that the valve of the aorta was thicker, harder and very much contracted; the symptoms explained this to him, since at every contraction the blood must flow back into the ventricle. His complete works were printed by Palmer in 1834.

Hunter's greatness was not recognized by his contemporaries. He valued ideas for their own value, quite independent of all other considerations, for which the English had no inclination so long as no definite result appeared. Shrewd, but short-sighted, they were unable to comprehend his far-reaching spec-

ulations. According to their opinion Hunter was little more than an innovator and fanatic, and even his practical improvements met but a cold reception. A secret antipathy to minds which far surpass our own everywhere lurks in human nature. The epoch-making work on inflammation, in which the subtlest and most difficult subjects in nature were treated, could gain no popularity for Hunter, because there were so few physicians who were able to follow him on this path. "The great Scotchman, thrown among a people whose mental habits were so unequal to his own, had," as Abernethy says, "a solitary and comfortless place by reason of his transcendency." They so little understood him, that during the four years when he lectured in London on anatomy and surgery, his hearers numbered twenty. If his own countrymen did not appreciate Hunter's greatness, what wonder that the same was true in Germany! A. G. Richter considered that in the work on the blood and inflammation, which did not reach his expectations, Hunter was, indeed, a clear-sighted observer with many new ideas; but he found that experience was often strained in order to conform it to theories. He had proven himself an excellent master of the art of expanding really useful, practical matters into a mass of empty theories, and of so analyzing every-day affairs that they became quite unrecognizable and were readily regarded as something altogether new. He could not write concisely and his prolixity and repetitions would tire even the most patient reader; he showed also, with his vigorous imagination, a great effort to present singularities. Richter would not contradict his experience with phlebitis, but he had never seen anything which seemed to confirm it, and did not consider it proven. But he had the greatest appreciation of the works on the teeth and on syphilis, although here, too, it seemed to him that Hunter had a great love for singularities, in that he often contradicted himself. Baldinger found the chapter on anuresis paradoxic. The translator, Hebenstreit, likewise emphasized the tendency to paradoxic statements; and found a reason for the lack of connection in the fact that Hunter had not enjoyed a liberal education in his youth, and only became a writer late in life(!). The article on the digestion of the stomach Tode considered a droll dream, and hoped that the English devisers of theories would gradually protect us from the prevalent anglomania. His work on the teeth was horribly dry and much in it was old; and for heaven's sake, let no one dare to confuse John Hunter with the celebrated William, for the former was a veritable Nimrod in hypotheses, as is well known. John Hunter was too far ahead of his time.

Only in passing will we cast a glance at the *surgeons in other foreign countries*, since most of them were greatly inferior to the French and English, and being little or not at all known in Germany, could not in the least exercise an equal influence on German surgery.

About the middle of the century a more active life began in the northern countries; they built hospitals and lying-in houses. *Sweden* at that time possessed the "northern Desault" in *Olof Acrel*, professor in Stockholm. Trained in Paris and in the French army, he first brought scientific surgery into his fatherland, and did an extraordinary amount for the improvement of the hospitals whose director-general he was. His "Surgical Cases" (1775), into which a large experience is gathered, was the first fruit of the new hospitals in Stockholm. He banished excessive warmth from the wards and strenuously opposed the many superfluous instruments, and the misuse of salves and plasters. *Bierchen* became known through his researches in cancer, wherein he sought to establish the diagnosis of scrofula and syphilis; *Martin* was known through his thermometric measurements; *Schützer* and the oculist *Odhelius* may be mentioned.

Denmark was not backward. Copenhagen built its Friedrich's Hospital. A society of physicians and surgeons was organized in 1772, under Tode, which, elevated to a royal society, began to publish the "Soc. med. Havniensis Collect." Although a surgeon at a salary of 150 thalers was maintained at that time in each district of three to six (German) miles, yet the standing of the profession was low, and it is related as something extraordinary that a Count von Reventlow made the surgeon of his county his physician, entertained him at his table and at his social parties. King Christian VII. took the most important step in the advancement of surgery when he founded a Royal Academy of Surgery in Copenhagen in 1785, in order to train better surgeons for the country and the army. They took the new academy in Vienna as a model, and here also, as was the case in Vienna, they involved themselves in a quarrel with the sorely offended physicians, which was continued for several years, and on the part of the surgeons in a very stormy manner. The academy was independent of the university and of the collegium medicum; its professors (three ordinary and four reserve surgeons) enjoyed the same rank as the university teachers and belonged to number eight of the fifth class, where they stood on a level with the gentlemen, between major and captain of horse. The lectures were given free, and in the Danish and German languages. The most eminent Danish surgeon was *Callisen*. He had risen from the barber shop and had served as steward and in Friedrich's Hospital, had then studied in France and England for several years, and had finally become professor, chief surgeon of the Danish fleet and director-general of the academy. Beloved as a teacher and popular as a practitioner, he deserves great credit for improvement in the medical system, and with his "Institutes and Principles" (a systematic treatment of surgery), won much applause. The learned *Heuermann* wrote a good manual on operating, and Professor *Scheel* a great work on the transfusion of blood and injection of drugs, giving his own experiments.

The time of the great Boerhaave had aroused a general scientific interest in *Holland*, and had brought about a more intimate connection between surgery and medicine than was the case in other countries. Therefore, the best Dutch surgeons of the seventeenth century were at the same time good physicians, and showed more interest in the pathologic bearing in surgery than in the mechanical part. For the rest, even until the middle of the last century, the same altercations between physicians and surgeons as to the limits of their respective departments took place, as in Germany and France. The Dutch authorities decreed, among other things, that no physician, but surgeons alone, should practice obstetrics. Along with the anatomist Albinus, above all others ranked the genial *Peter Camper* (1722–1789), who was equally celebrated as an anatomist and physiologist and as a surgeon. At school, he employed his leisure time with architecture, optics, the turner's trade and cabinet-making, and after completing his studies,

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became on the same day Doctor of Medicine and Doctor of Philosophy. Repeated travels to England, France, Switzerland, and Germany brought him in contact with the most illustrious men of his time. In 1750 he first became professor in Franecker, then engaged in anatomy and surgery in Amsterdam, later in Gröningen, where besides these sciences he taught botany and medicine, but between his courses always went back to his estate in the country. Finally he settled in The Hague, where as a common councilman he died. Camper showed unlimited intellectual penetration; he was a master in drawing, painting in oils, copper engraving, turning, wood carving, and to all this he added a great knowledge of mathematics. When the war in Holland incited him to a study of fortifications, the Dutch canals were strengthened against the sea according to his plans. He also mastered the literature of every department of knowledge, especially the history of art, and was acquainted with both ancient and modern languages. He was a patriot in the noblest sense, was always ready for pleasure, and on the whole a very amiable man. Camper's scientific activity covered all branches of natural science, especially anatomy and zoology, in which, for example, he spared no expense to have a rhinoceros brought from Africa or a whale's head from Greenland, in order to study the anatomy of the hearing organ. His most important works in surgery were his "Demonstr. anat. pathol." (2 Vols, 1760–1762). He wrote on the diseases of the arm and pelvis, separation of the os pubis, cancer of the breast, lithotomy, fistula of the anus, prolapsus of the anus, bone callus, causes of lameness in children, fracture of the patella and of the olecranon, and congenital hernia, and made drawings of intestinal rupture. Few scholars have written so many prize essays as he; the Académie de Chirurgie in Paris five times awarded him a gold medal for his works on the misuse of salves and plasters, the best form of truss, the influence on surgical diseases of different kinds of air and of waking and sleeping. He wrote an original work on the best shape of shoes (1781), which showed the anatomic disadvantages of a high heel and was translated into various languages. There was no other Dutchman who approached Camper. *Van Gesscher* defended the necessity of amputation against Bilguer, and wrote on curvature of the spine. Professor *Bonn* in Amsterdam wrote on callus, luxations, bladder incisions (which he recommended to be made above the os pubis), and was also the author of "de continuationibus membranarum," from which Bichat borrowed a great deal, word for word, without giving credit. *Van Der Haar*, and *Ten Hoaff* the ship surgeon, and *Van Wy* the lithotomist of Amsterdam, should be mentioned.

In *Italy* the last century produced a few good surgeons, but no epoch-making ones, until the last two decades, when A. Scarpa taught. When in the middle of the century the Italian language was so popular in protestant Germany that students in Halle had their theses printed in Italian under the direction of their language teacher, medical and surgical writings began also to be accessible through translations ("Eschenbach's Choice of Best Essays," 1783, "Kühn & Weigel's Bibliotheque," 1793). To the Italians as well as to the Germans of that time Paris and London were the El Dorado of all science. Among those who are still remembered was *Guattani* in Rome, who through his monograph on aneurysms ("De Externis Aneur." 1772) is erroneously regarded as the actual author of indirect compression (see Leber); he wrote the first accurate introduction to esophagotomy, giving experiments made on animals. The name of *Vacca Berlinghieri*, director of the surgical clinic of Pisa, is attached to this operation through the discovery of a special knife. Professor *Bertrandi* deserves credit for the reform of medical study in the Sardinian States; he wrote a manual on operating. In operations he laid stress upon accident, which sometimes made the most difficult ones a success and the simplest a failure. In Naples Professor *Cotunni* discovered the water passages in the interior of the ear (1761), gave exudation of the lymph in the walls of the N. ischiadicus, or the N. cruralis as the cause of ischias and recommended, accordingly, a strong vesicant along the nerve (1764). *Pajola* was considered a skilful lithotomist in Venice, as was also the Papal body-surgeon, *Flajani* in Sassia, whose collection of bladder-stones was one of the finest in Europe. In Florence lived *Benevoli*, *Palucci* (later in Vienna), *Cavallini* and *A. Nannoni*, who introduced a milder and less painful treatment of surgical diseases into Italy (1761), and also contended sharply against the abuse of salves and plasters. In the hospital in Milan worked *Paletta*, *Monteggia* and *Moscati*, later body-physician of Viceroy Eugene, and who became known through his assertion that man was intended to walk upon four feet. Vrolik in Amsterdam wrote a special dissertation to combat this evolution view. *Nessi* was trained in Pavia and was the author of a handbook on surgery, translated into German; and Pavia produced the most famous of all the Italians, *Anton Scarpa*. He was a pupil and assistant of Morgagni when the latter became blind; he was professor of anatomy and surgery in Modena and after 1784 in Pavia, where he established a surgical clinic. Napoleon, as king of Italy, appointed him body-surgeon and until 1812 he conducted the clinic; then on account of weakness of sight, being otherwise very vigorous (which he attributed to hunting), he retired. He died highly honored at the age of 85 in the year 1832. His greatest service was the development of surgical anatomy, which gave a characteristic stamp to all his works; these were provided with the most excellent drawings. We owe to him a so-called boot for club-foot (with which the patient could walk, 1803), some excellent works on aneurysms (1804), hernia (1809) and diseases of the eye.

The most important occurrence in *Spain* was the founding of an academy of medicine in Madrid (1732) and of a separate college of surgeons (1741). At the end of the century there were sixteen medical and three surgical schools (Madrid, Barcelona and Cadiz). The study of medicine, for which no great preparatory knowlege was demanded, was mostly theoretic; the professors lectured and were commanded by the Castilian council to expound the institutions of Boerhaave. After a surprisingly easy doctor's examination a still further examination in Madrid, costing sixty thalers, was required for permission to practice. A supreme court placed over all Spanish physicians ruled with a despotism which debased their position; without assigning reasons it imposed fines and revoked the license to practice. The consequence was that only people from the lower classes, without means or education, studied medicine. Nevertheless, the number of physicians was very large; every village possessed a young Æsculapius (in Madrid, with 160,000 inhabitants, there were 135) so that almost all were

poor and very few could support their families. Frequently they went as beggars, on foot, from place to place and begged at the doors of monasteries for some soup and a little alms. Under such conditions it could be no great triumph for the surgeons when they, through their influence at court, obtained political equality with the physicians made evident externally by being allowed the Spanish cockade and lace-trimmed clothes and receiving military rank. In 1795 a medical school was established in Madrid with better principles of instruction and entrance was made dependent upon a better elementary education. From that time on everyone who wished to practice in Madrid had to be a doctor and pay 300 thalers. One of the most distinguished among the Spanish surgeons, who were chiefly educated in the French schools (des Nähere bei Ullersberger, *Deutsche Ztschr. f. Chir.*, ii, 2:3, 1873), was *Anton de Gimbernat*, who first discovered (1793) the cause of the strangulation of the crural rupture in the part of the crural arch called after him.

(To be continued.)

PRACTICAL NOTES.

Congenital Teeth, according to Dr. Ballantyne of Edinburgh, Scotland, as being ill developed and serving no normal purpose in the economy, are best removed soon after birth. These exceptions are exceedingly rare conditions, for out of Magitat's 500 cases in which the eruption of the first tooth was noted, in only 1 case were there teeth at birth. Further, it is recorded that out of 17,578 new-born infants at the Paris Maternity, in ten consecutive years, only 3 had teeth, or not much more than 1 in 6,000. These teeth, as may be foreseen, have little or no significance as regards the bodily or mental vigor of the infant carrying them. Certainly we may add they are not stigmata of a degeneration into savagery and are not to be classed as freaks of development. Probably Shakespeare had much to do with fixing the superstition of a monstrosity in the popular mind by one of his best-known dramas.

An Epidemic of Infantile Paralysis.—Dr. W. Pasteur of London, before the Clinical Society of that city, read a paper on an epidemic of infantile paralysis occurring among children of the same family. The epidemic was limited to this household. Every one of the seven children in the family were attacked in rapid succession, within the space of three weeks, with fever of moderate severity, accompanied by severe headache. In three of the children, aged 11, 9 and 5 years respectively, paralytic symptoms supervened within seven days of the commencement of illness. The type of paralysis varied in the different cases. In one there was flaccid paralysis of the left arm; in another right hemiplegia with marked and persistent muscular rigidity of the arm and the leg, with more transient paralysis of the face and soft palate on the same side; while, in the third case, there was paralysis with rigidity limited to the left lower limb. In two more the primary fever was followed by tremors lasting a few days, while in the remaining two the primary fever was not followed by any nervous disturbance. As regards the cause of the outbreak, it was possible to exclude the acute exanthemata, diphtheria and influenza. The points of resemblance to infantile paralysis were held to justify the diagnosis in spite of the remarkable characters of the paralysis. The cases prove that a poison giving rise in one instance to a paralysis indistinguishable from the acute atrophic paralysis of childhood is also able to determine lesions in other parts of the nervous system. They further strongly confirm the growing impression that infantile paralysis is an acute infective disease.

Acetanilid and Boric Acid as a Surgical Dressing.—Dr. Fallas writes (*Medical News*) that he had been led to seek for a combination with acetanild which would fully inhibit suppuration. He first tried boric acid, acetanilid and carbolic acid, but the powders were not sufficiently absorptive, and the compound was too moist and pasty. After many trials he used this: Powdered acetanilid, 48 per cent.; powdered boric acid, 15 per cent.; powdered starch, finely sifted, 35 per cent.; carbolic acid, liquid, 2 per cent. This compound, though slightly moist and adhesive, has all the effect of a dry dressing; in fact, upon a wound it is, or becomes, perfectly dry. Its absolute comfort leaves nothing to be desired; under its use there is not from first to last the least discomfort, except a momentary smarting on the first application, too trivial to be worth mentioning. The most exquisitely sensitive sore can in a few days be handled with impunity. There is no odor, even of the carbolic acid. It absolutely inhibits suppuration where it can reach the wound surface, and it checks and quickly abolishes suppuration if that be already present. The powder should be changed twice a day, as long as there is a discharge to moisten it; soon, however, the wound becomes absolutely dry, and the powder may be left undisturbed for many days, and will still be found as dry as when applied, unless there is a pocket from which pus may come. Cicatrization is rapid and satisfactory. Obviously, this powder would be unsuitable for introduction into a cavity, unless it was intended that the cavity should granulate from the bottom; and in that case the possibility of easy removal of the masses formed by the pus soaking into the powder before the wound becomes dry, should be considered. On an accessible granulating surface and upon a sutured wound, it comes very near to being a perfect dressing.

Placenta Previa at the German Congress of Gynecology.—This subject was the question of the day at the recent Congress and Küstner stated that he had delivered eleven living infants out of sixteen cases of placenta previa treated with the colpeurynter. He adds that 500 grams are enough to distend it, and hoped that some instrument would be devised soon to perforate a "central" placenta without injury to the large vessels or to the child. Several observations were described which prove the existence of a cervical placenta previa. Schatz observed that if there is a connection between placenta previa and endometritis, as seems probable, the latter should receive careful treatment whenever it occurs, and the former would then be less frequent. If it could be diagnosed in the early stages, then artificial abortion and curettement would be indicated, but it is scarcely possible to diagnose it at first, although with skill and experience it can be detected at the third month. He orders repose, morphin and viburnum prunifolium, in case of hemorrhage during the latter months, continuing the last a long while. If hemorrhage persists, he tampons the vagina until able to practice the Braxton Hicks version. He does not rupture the bag of waters until the head is engaged in the pelvis or podalic version can be effected. He has not found the colpeurynter useful except after the rupture, when he inserted it into the uterus, with gentle traction on the tube. He tampons the uterus finally, if it does not contract sufficiently. Hofmeier remarked in his address that persistence of the hemorrhage after rupture of the waters is an indication in favor of a normal placenta prematurely detached. He disproves of leaving the vagina tamponed several days, and ruptures the bag as soon as labor has commenced, as the fetus pressing against the cervix will compress the bleeding parts. He follows it immediately with combined version and drawing down the feet. He recommended also the Braxton Hicks method, using the colpeurynter if the dilation is insufficient. The strength of the patient must be sustained with subcutaneous injections of salt solution, ether, or camphorated oil, injections of stimulants, etc. When impossible to reach the edge of the

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placenta, rupture of the bag and podalic version by tearing the chorion and amnion is the only resource. The child has usually to be sacrificed, but there are scarcely any means to save it in these conditions.

The Value of an Exclusively Meat Diet in Certain Cases of Chronic Gout.—At a recent meeting of the Medical Society of London, Armstrong (*British Medical Journal*, May 1, 1897) related that during the last few years he had given trial in various cases of gouty arthritis and recurrent renal calculi to a method of treatment based on the prolonged administration of only red meat and hot water. Very marked improvement had resulted, which had persisted in spite of gradual return to an ordinary dietary. His own observations on auto-intoxication in relation to the causation of gout and rheumatoid arthritis, had led him to believe that it is the complex chemic changes brought about by the admixture of red meats with carbohydrates and sugar that causes the excessive formation of uric acid. His plan is to give the patient a daily allowance of from one to four pounds of lean beefsteak, minced and cooked in various ways, the patient drinking from one to five pints of hot water, and avoiding all starchy, saccharin and fermentative articles of food. The effects observed are rapid diminution in abdominal girth, loss of fat and flabby tissue, increase in the quantity, and ultimate disappearance of urates, alkaloids and toxins from the urine. The joint-swelling greatly diminished, the aching and pain became much lessened and the mobility of the joints was considerably increased. The capacity for work, mental and bodily, was enhanced and the symptoms of mal-digestion and disassimilation disappeared. This treatment is indicated in obstinate chronic gouty arthritis, in recurrent uric acid calculi, in frequent and intractable migraine, and in cases of persisent gouty dyspepsia. It has proved especially useful in the presence of symptoms of amylaceous and intestinal dyspepsia and of excessive formation of hydrogen sulphid, urates, indican, skatol, creosol and other toxins. The carbohydrates, by their affinity for oxygen, interfere with the due oxidation of the tissues. Under the plan of treatment proposed, digestion is more complete and the processes of fermentation and putrefaction are kept within narrower limits, oxidation being more perfect and less waste being consequently left in the system. Whatever poisonous matters remain in the system are readily eliminated by the taking of the hot water, which also flushes the stomach, liver and kidneys. The treatment is irksome and trying, and must of necessity be carried out with great strictness; therefore its use should be confined to the more difficult cases. It should be prescribed but rarely and then only under the most careful supervision in cases in which the heart or kidneys are diseased. Used with due care it is a most efficient and brilliant addition to the therapeutic measures. In the discussion Haig admitted that certain cases of gout are benefited by a meat diet, as a result of its stimulating qualities. When it fails to act as a stimulant it is likely to prove dangerous.

Case of Fibrinuria.—The *British Medical Journal* quotes Dr. A. Klein's report, in a Vienna journal, of one of these rare cases in which fibrinous clots are passed in urine. Klein first analyzes the four recorded cases that had been previously recorded regarding this condition. The author's case was that of a male, aged 52 years, who had been ill for fifteen months with cough, headache and dyspnea on exertion, to which had recently been added swelling of the extremities and rigors. Examination led to the diagnosis of Bright's disease; the urine was acid, of specific gravity 1013, and contained a large quantity of albumin, with numerous hyaline and epithelial casts. Treatment consisted in hot baths and milk diet, and the patient improved considerably, the edema almost completely disappearing. Ten days after admission it was noticed that the urine, which was slightly alkaline, contained a large number of grayish-white clots, some rounded and others flattened, and varying in length from half an inch to four inches; some were as much as two inches thick. These, if allowed to stand, settled to the bottom, and the supernatant urine sometimes showed a further curious phenomenon in the shape of fine threads, which formed a network extending throughout the fluid. On shaking, this became detached from the sides of the vessels and formed a well marked clot; the urine had thus clotted just like a serous fluid. The amount of deposit remained constant, except just before a rigor, when it diminished to return to its former amount with the subsidence of the temperature. If the clot were allowed to remain in the urine for a few hours it disappeared entirely, probably by a sort of process of self-digestion. Chemic examination proved the clot to be composed of fibrin. The urine from which it was deposited showed, as compared with that at other times, alkalinity, much diminution in solid content, an especially minute amount of phosphates, but an enormous quantity of albumin. There could be no doubt as to the renal origin of the fibrin, but it was very hard to define its pathologic cause. Evidences of nephrolithiasis, kidney abscess, etc., which had been present in former cases were here wanting; nor did hematuria ever appear. Klein attributes the coagulation to the alkaline reaction, the large quantity of albumin, and the almost complete absence of phosphates; it bore also, no doubt, some relation to the sudden rigors and rises of temperature. Some time after the completion of the experiments the patient became worse and died; the kidneys were found to be in a condition of amyloid degeneration, and their tubules contained hyaline masses and threads giving Weigert's reaction for fibrin. Klein adds the notes of analyses of two other kinds of clots. The one was found in the urine in a case of cystitis; it consisted of a nucleo-albumin inclosed in a capsule of mucin. The other was a cast from a patient suffering from plastic bronchitis, and this failed to give the fibrin reaction either in bulk or in section; it consisted of mucin, and contained large colonies of diplococci.

A Rapid Method of Fixing and Staining Blood-Films.—Gulland (*British Medical Journal*, March 13, 1897, p. 652) describes briefly a method of making permanent microscopic preparations of blood that he has learned to follow and that gives accurate fixation, is rapid in execution and is sufficiently flexible to avoid the spoiling of preparations in consequence of even considerable deviations from the exact method. A small drop of blood, drawn in the usual way, taken up on the center of a cover-glass held with forceps and distributed evenly between that and another cover. The utmost care must be taken to avoid all pressure, as the after-appearance of the red corpuscles depends almost entirely on the way in which this maneuver is carried out. The covers are then gently and rapidly slid off one another and dropped, with the wet side downward, into the fixing solution, which consists of absolute alcohol saturated with eosin 25 c.c., pure ether 25 c.c., mercuric chlorid in alcohol absolute (2 gm. to 10 c.c.) 5 drops (more or less). The quantity required for use at one time, which may be from 5 to 10 c.c. for four cover glasses, should be poured into a wide-mouthed bottle or a flat dish, and may be used several times over if it be preserved from evaporation. The three liquids may be kept in separate bottles and be combined in the required proportions when needed. The fixation of the elements is practically instantaneous, but the cover-glasses should be allowed to remain in the solution for at least three or four minutes, in order to fix the film to the cover. They are then taken from the solution with forceps and washed rapidly but thoroughly by waving them to and fro in a small basin of water. They are then stained for one minute (not longer) in a saturated watery solution of methylene-blue and again rapidly washed in water. Next, they are quickly dehydrated in absolute alcohol (which at the same time removes the excess of methylene-blue), cleared in xylol and mounted in xylol balsam on a slide. The whole process need not occupy more than six or seven minutes, although any portion of it may be prolonged without injury to the specimen. The fixation may be continued for twenty four hours and the washing for the same time,

but if the staining with methylene-blue be prolonged for more than a minute or two it becomes necessary to use an inconveniently large amount of absolute alcohol to remove the excess of the stain, and the eosin may be washed out at the same time. The red corpuscles are stained pink, the nuclei a deep blue, the bodies of the leucocytes various shades of pink; the eosinophile and the basophile granules in the leucocytes are well brought out; the blood-plates are stained a fainter blue than the nuclei; and organisms also are well stained. Any other stain that is soluble in alcohol and is not precipitated by mercuric chlorid may be used instead of eosin; and the stain may be omitted from the fixative altogether, so that the coverglass after fixation in the alcohol-ether-mercuric-chlorid solution may be stained in any way that is desired. The method is also useful for fixing pus, sputum and anything else that may be spread in a film; only with these it is advisable to prolong the fixation.

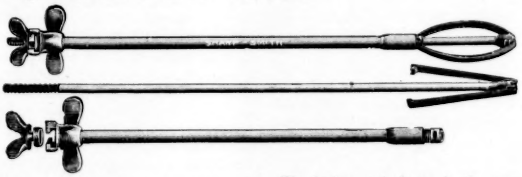
Gunshot Wounds of the Chest with Penetration of the Lung; Extraction of Bullets; Recovery.—Musgrove (*British Medical Journal*, May 29, 1897, p. 1342) has reported the case of a man, 26 years old, who was found in a field in a state of collapse, bleeding profusely from the front of the chest. He was conscious and stated that five and a half hours before he had fired two bullets from a revolver into his chest, with the intention of killing himself. On removing the clothes saturated with blood, after transporting the man to his home, two circular wounds were found in the precordium, one over the fourth rib and the other in the fourth interspace, both blackened with gunpowder and both bleeding profusely. From the lower wound also air was escaping at each expiration, whilst there was emphysema of the cellular tissues on the left side, extending up into the neck and down to the level of the lower ribs. On probing these wounds the track of the upper one was found to pass outward along the rib, while that of the lower led almost directly backward, with a slight inclination to the left. The probe was passed along the track of the lower wound for six inches backward through the cavity of the chest. No bullet could be felt in either of the wounds. On account of the weak condition of the patient no further search was possible until a little later, when by turning the patient upon his right side and making incisions along the inner border of the scapula and using a probe the two bullets were found, the one corresponding to the upper wound in front lying in contact with the outer aspect of the fourth rib, along which it had evidently run, while the other lay projecting between two ribs and had to be dragged forcibly out by forceps, along with a piece of bone (apparently from the anterior part of the fourth rib) with which there were entangled fragments of wool, corresponding in color and texture to those of his undershirt. These bullets were of large size and had sharp projecting spiral spurs on their surface, evidently due to their having been too large for the bore of the revolver. As ether and chloroform were inadmissible, owing to the patient's condition, local anesthesia was effected by means of ethyl chlorid spray and hypodermic injection of cocain. The incisions were afterward packed with boric acid lint soaked in carbolic lotion. For three days the respirations were rapid, but the pyrexia was slight. On the second day an attack of severe dyspnea occurred. There was no expectoration or cough until the fourth day, when a small quantity of plum-colored sputum was brought up and also once or twice daily until the tenth day. On the first appearance of the sputum the odor of the breath became offensive, but it became normal again in the course of a few days under the use of a creosote spray. No lung-tissue was found in the sputum. The incisions in the back were dressed daily, being packed with lint as at first. From the fourth to the sixth day a rubber drainage-tube was substituted, but was subsequently discarded for the former. On about the fifth day dulness and other signs of pleuritic effusion on the left side appeared, and on the twenty-third day of the illness, when this dulness had risen to the level of the spine of the scapula an exploratory trocar was introduced and four ounces of transparent blood-stained fluid withdrawn. On the repetition of this operation a week later no fluid could be obtained, but the costal pleura was felt to be enormously thickened. The dulness cleared up, leaving a slight impairment of the percussion note. The posterior incisions healed up in the course of a month, and several weeks later the patient was able to take a long walk without dyspnea or fatigue.

NEW INSTRUMENTS.

A NEW CURETTE.

BY COURTNEY L. SMITH, M.D.
AURORA, ILL.

Curetting the uterus for endometritis, subinvolution, retained products of conception, etc., has come to be a very common operation. It is deservedly recognized as a valuable procedure in these conditions, and accomplishes speedily what local treatment or the exhibition of medicines can not. It needs no argument to prove that unless curetting is thoroughly done, and the material scraped off is removed from the cavity of the uterus, the operation is apt to do more harm than good. I think the experience of the majority of physicians will bear me out in the statement that there is a large element of uncertainty in performing curettage with the ordinary spoon curette. We are never sure, in using this instrument, that we have not neglected to reach some portion of the mucous surface, or that some shreds of tissue have not been left behind to cause trouble.



The instrument shown in the annexed cut was designed to overcome these objections and adds much to the ease of performance and the safety of the operation. The main part of the instrument consists of two cutting or scraping blades, which can be stretched flat or bowed out in the form of an ellipse by a rod and screw. After dilatation of the cervix in the usual way by a branched dilator, the closed curette is readily slipped up to the fundus. The screw is now turned, thereby separating the scraping blades and drawing the stretched membrane tight down upon them. Rotation of the instrument removes the diseased tissue, much or little being scraped away as we desire. A backward turn of the screw straightens the blades and the instrument is withdrawn.

An examination will now show that the space between the blades is filled with shreds of tissue brought out with the instrument. A plunge in sterilized water cleanses it, it is again introduced slightly dilated and given a turn or two and drawn out with the remaining shreds. The products of the operation are now removed with the exception of a slight amount of blood and small clots, which are readily washed out by the irrigator.

The advantages claimed for the instrument are the following:

1. It is easily introduced and manipulated.
2. Every portion of the uterine mucosa is curetted as the instrument is revolved.
3. As it is withdrawn all shreds of tissue are removed with it. And
4. There being no sharp angles or points to go through softened tissues, there is no danger of perforating the uterine wall.

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SATURDAY, AUGUST 14, 1897.

THE ANTIVIVISECTIONISTS AND THEIR LITTLE BILL.

Whether or not we may console ourselves with the thought that many of those posing as antivivisectionists are degenerates or perverts, we must remember that Senate Bill No. 1063 is still on the calendar and will undoubtedly be taken up at the next session. Missionary work must be done during the recess of Congress. Here the general practitioner may be of infinite service in giving his Senators and Representatives, wherever they are found, his opinion why the bill should not become a law. The infamous attempt to stifle physiologic experiments by the enactment of the bill can be defeated, if the profession will see to it that each Senator and Member of the House of Representatives is furnished with the necessary reasons why the measure should not pass.

Modern medical science rests almost exclusively for its facts upon animal experimentation; without it the beneficent effects of vaccination would have been unknown, physiology would have been a collection of fables, and bacteriology still in the future. The great practical benefit to mankind through the advanced state of knowledge in medicine and surgery should be made clear to our legislators, and the effect of passing crippling laws should be clearly explained. The bill can not be beaten by making faces at the misguided perverts or even by pointing out their lack of mental equilibrium; we must demonstrate fully and conclusively the evils that will flow from the enactment of a law stopping animal experimentation; that man who is a little lower than the angels, is yet immensely superior to the lower animals, and that the same law of self-preservation that causes us to slay and eat, causes the humanitarian to use animals to work out problems for the prevention of the disease, through development of human wisdom, for the preservation of the human species.

The higher value placed upon human life is the glory of modern civilization, and notwithstanding the existing necessity for enslavement and use of the lower animals we are far kinder to them than the ancients. The ancients, who roasted their enemies, tore them limb from limb, harrowed them with "harrows of iron," tortured them with fire, made them beasts of burden when great public works were to be erected, were even more cruel and careless of the lives of animals. If they occasionally worshiped an animal, it was on account of some particular benefit conferred, or dread of evil to come, a habit which JUVENAL set forth with his pungent sarcasm.

> The serpent-eating ibis these inshrine;
> Those think the crocodile alone divine;
> Then in another place you may behold
> The statue of a monkey shine with gold.
> Here men some monstrous fish's aid implore,
> And there whole towns a grinning dog adore.
>
> —JUV. XV. 2.

Nearly all that was known of anatomy in the early times was obtained by dissection of living and dead animals. By command of ALEXANDER the Great, ARISTOTLE made regular vivisections on animals, and later PTOLEMY PHILADELPHUS, apparently to outdo ALEXANDER, gave HEROPHILUS and ERASISTRATUS permission to dissect criminals condemned to death, and a great number were used in that way, but so far as history informs us, no subsequent ruler ever gave permission of this sort, and through all the centuries down to the present time, animals have been used to increase the stock of human knowledge by every principal investigator in the field of anatomy and physiology. Now, by reason of increased knowledge brought by animal experimentation, serum and toxin therapeutics, we in this generation have gone a step further and have discovered means of arrest and cure of some of the worst scourges known to mankind. Shall the cranks prevail and by creating false sentiment succeed in preventing the further development of bacteriology? We trust not brethren, but *let us beat Senate bill No. 1063!*

NOSTRUMS AND PHARMACISTS.

The report of a committee on a prize for an essay on price-cutting by druggists (signed by the dean of the Chicago College of Pharmacy, by a member of the American Pharmaceutical Association, by a wholesale jobber in drugs who largely advertises abortifacient nostrums, and by the "promoter" of a proprietary "tonic" advertised only in the daily press), announces that the highest achievement of the pharmacal skill of the close of the nineteenth century, is the nostrum, and eulogizes this "grand result" of

"scientific" acumen in the fulsome style so characteristic of the "promoters" of dubious enterprises. This fulsome eulogy which seriously reflects on hundreds of reputable pharmacists is as unjust to them as it is historically false. CRABBE, nearly a century ago, sang anent patent medicines in "The Borough":

> With monstrous promise they delude the mind
> And thrive on all that tortures human kind;
> Void of all honor, avaricious rash,
> The daring tribe compound their boasted trash;
> Tincture or syrup, lotion, drop or pill,
> All tempt the sick to trust the lying bill,
> And twenty names of cobblers turned to squires,
> Aid the bold language of these blushless liars.
> How strange to add, in this nefarious trade,
> That men of parts are dupes by dunces made;
> That creatures, nature meant to clean the streets
> Have purchased lands and mansions, parks and seats;
> No class escapes them, from the poor man's pay
> The nostrum takes no trifling part away.

The patent medicine traffic herein commemorated had, however, begun more than half a century previously. In 1711 in New Jersey a Mrs. MCMASTER sold "Tuscarora Rice a Cure for Consumption." Concerning the eighteenth century patent medicine trade in other English-speaking countries Mr. ALPE states (*Pharm. Journal* and Transactions, 1887), that the *Gentleman's Magazine* for 1748, contained a "Pharmacopeia Empirica," which shows that patent medicines were exceedingly numerous in England in the eighteenth century. The "Pharmacopeia Empirica" was compiled that "persons who could not afford the expense or might not care to be governed by the advice of the physician and apothecary, might know where to apply on occasion for an appropriate remedy." It contained 202 patent medicines, 90 per cent. of which were manufactured in London. This list, therefore, was far from complete. Patent medicines for gleet, gonorrhea, syphilis, impotence and sterility are exceedingly numerous. One man advertises "belts" for cure of "leprosy," at $1.50 each. "Necklaces" for "cutting teeth" are advertised $1.25. "Girdles" for the "itch" are advertised at exorbitant prices. Among the patent medicines mentioned are "James's fever powders" at $1 per "paper;" "Godfrey's cordial" at 75 cents a bottle; "Daffy's elixir" at 63 cents; "Bateman's drops" at 25 cents; "Stoughton's bitters," and "Betton's British oil." The highest priced articles are "Dr. Bellost's pills" for rheumatism, at $5.00 per box, and "Profely's pills" for syphilis at $2.50.

Mr. ALPE gives a list of patents for medicines, beginning with one in 1711 by TIMOTHY BYFIELD, described as "A new and most vsefull chemicall preparation and medicine publickly known" by the name of his "sal oleosum volatile which, by abundant experience, hath been found very helpful and beneficial as well in uses medicinall as others and will very much tend to the public use and benefit of our subjects." ROBERT STOUGHTON patented his "elixir magnum stomachi" in 1712 (now surviving as a bar room bitters). The next patent was in 1722, obtained by ROBERT EATON for a "styptic" medicine. BENJAMIN OKELL came next (1726) with "Dr. Bateman's pectoral drops." "Betton's British oil" was patented in 1742; "Hooper's female pills" in 1743; and "James's fever powder" in 1744. About this date the patenting of medicinal compounds became more common—three or four in a year.

Some of these display polypharmaceutic tendencies. THOMAS GREENOUGH got a patent in 1744 for a toothache tincture containing twenty-six ingredients. NATHANIEL GODBOLD obtained one in 1785 for a vegetable balsam, into which he had worked fifty-two ingredients. In 1805 RICHARD BRANDRETH the elder was given a patent for some constitutional pills with ninety-one ingredients. Mr. GODBOLD patented his balsam in 1785, and "improved" upon it in 1798. "John Ching's worm lozengers" were patented in 1796, and "improved" in 1808 by REBECCA CHING. The object of the "improvement" was to evade the time limitations of the patent laws. Some modern medicinal preparations seemingly originated in these patents which, however, were often prescriptions of physicians stolen, "improved" and patented. "Friar's balsam," according to Mr. ALPE, originated as a patent obtained in 1777, by ROBERT GRUBB, for a certain medicine called the "Friar's drop for cure of venereal disease, scurvy, rheumatism, strangury and gleet, without any electuary or pills, in every stage of the complaint, or without any confinement or particular regimen." To make the "drops" rectified spirits are added to calomel, purging anatomy, guiacum wood, balsam Peru, extracts of hemlock, sugar candy, oil sassafras, tartaric acid, and gum Arabic.

Patent nostrums are fetich survivals of polypharmacy of the medicine men. Their atavistic persistence in the nineteenth century is a reflection alike on popular intelligence and commercial honesty. The signature of the dean of a pharmacy college to such a fulsome endorsement as that described can not but be regarded as evidence of unfitness for the position of teacher. The AMERICAN MEDICAL ASSOCIATION, by creation of its Section of Pharmacy, has endeavored to elevate the status of the pharmacist. Performances like these render the performers (if a member of the American Pharmaceutical Association) *persona non grata* as a delegate to the Section on Materia Medica and Pharmacy of the AMERICAN MEDICAL ASSOCIATION.

HIS ROYAL HIGHNESS, THE PRINCE OF WALES, F.R.C.P.

The London *Lancet*, of July 24, devotes an editorial to an explanation of the significance, as it understands it, of the recent conferring of an honorary fellowship of the Royal College of Physicians on the Prince of Wales. Judging from the tone of this lead-

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ing editorial, for we have no other source of information, the appointment has been looked upon somewhat askance by certain members of the medical profession in Great Britain. The ordinary significance of an honorary degree is, that its recipient possesses eminently respectable, if not pre-eminent qualifications in the particular branch of learning represented by the degree, and to make such a bestowal upon one who has notoriously had no special training, solely on account of his official position, has upon its face a certain stamp of grotesque impropriety. The *Lancet* admits that the granting of an honorary medical title, no matter who is the recipient, will seem ridiculous to some, if he has had no medical training and can claim no medical knowledge, and in this country there is no doubt that such a proceeding would meet with general ridicule; and the learned body or corporation granting such a distinction would suffer more or less severely in public estimation. One can not here speak as an expert on the special impropriety of the action of making the heir to the British throne an honorary member of our profession, but it is evident, from the semi-apologetic character of the *Lancet's* leader, that something of the sort was more or less appreciated by our transatlantic brethren, and hinted if not freely and openly discussed. Some members of our profession evidently questioned whether it was materially elevated by making official position, even the highest, a qualification for its honors, and in a strictly medical and scientific point of view their doubts are amply justified. Our contemporary, however, probably states the more general and popular opinion of our British *confrères* when it says that, as representative of her Majesty the Queen, it is perfectly appropriate and germane to his position that he should enjoy high official status in all national institutions, including the higher professional corporations, and that his gracious action in the acceptance of the compliment (it does not venture to call it an honor) is, in its opinion, "a pleasing recognition of the whole profession of medicine."

In the light of the *Lancet's* editorial, and considering the peculiarities of British political and social institutions, we must admit that there is much force in its way of stating the case, and that the ridiculous aspect that is at first sight most obvious is very materially softened down. It would not take well in this country to have an honorary M.D. bestowed on President McKinley or Vice-president Hobart on the sole ground of their official rank, and neither they nor the profession would feel that honor had been bestowed or received; but circumstances are not the same in Great Britain, where so much depends on recognition by royalty and the aristocracy. One has only to glance at the rosters of English medical and charitable institutions to see how important such recognition and patronage is regarded. If this "pleasing recognition" of the medical profession will in any way or to any degree elevate or improve its social status in Great Britain, we in this country can only express our satisfaction with such a result. It is not so very long since this Journal had occasion to notice what appeared to be a lack of due recognition of the medical profession by the highest social arbiters in Great Britain, and again to notice some British exceptions to its criticisms. If we could take this acceptance of a medical honor by the Prince of Wales as in any way, directly or indirectly, intended as an answer to such criticism, the critics may congratulate themselves upon having had some influence in producing what the Journal trusts will be a benefit to our English *confrères*. The Journal would also congratulate H. R. H. the Prince of Wales on having been made an honorary member of the profession which above all other secular ones has cherished and maintained a high standard of honor and ethics, and which is second to none in its honorable history and traditions. Of all the various honors he has received he need not consider this among the least, even with full recognition of the fact that its conferment is solely due to the special conditions existing in a country where honors are hereditary and simple manhood or scientific merit are secondary matters as regards social consideration and standing.

KOCH'S EXPERIMENTS WITH THE RINDERPEST.

The recent issue of the first official reports of Koch's investigation of the rinderpest in South Africa is attracting much attention to this subject from the African and Indian bacteriologists and veterinarians; and there is strong reason to believe that his experiments in connection with the immunization of cattle from this deadly plague will result in the saving of millions of animals. The real import of Koch's work can only be understood when we consider that a low estimate of the annual monetary loss from cattle destruction alone, by this disease, is over thirty million dollars. There is an extra significance in the fact that this loss is sustained by a people generally poor in other than agricultural ownings.

The details of Koch's actual system of practical procedure are of such interest as to command the attention of every one interested in the development of the serum treatment of disease, and the following epitome is offered to our readers.

On Koch's arrival in South Africa in December, 1896, he found a belief among the Free State farmers that the bile of an animal dead from rinderpest, mixed with blood or other liquid, would cure the disease. Experiments with bile as a curative agent were negative. Koch then directed his attention to the inoculation of cattle with cultures of the alleged specific microörganisms of rinderpest. Such inoculations did not produce any symptoms of the pest, and

offered no protection. Having failed in confirming any of the so-called discoveries of the origin of rinderpest, experimentation was directed to the main object of the visit to discover a process of attenuation of the virus of rinderpest, so as to use it by inoculation as a certain preventive measure.

KOCH found that a mitigated rinderpest could be produced in sheep and goats, animals ordinarily less susceptible to the disease than cattle. Inoculations were continued to the seventh generation, and the later serums produced mild manifestations of the disease. In the hope that by passing the disease through the smaller animals it might be given to cattle in a mild but protective form, KOCH inoculated four heads of cattle with the virus of two sheep and two goats, after it had passed five times through the latter animals. The two cattle inoculated with the sheep virus exhibited a violent and fatal form of rinderpest, giving the inference that passage through the sheep intensified rather than lessened the strength of the virus. The results of the inoculations with the goat virus were more encouraging and further experiments were made with the goat virus exclusively, showing very slow, but actual attenuation of the infection by passage several times through the animal. None of the goats died, but one typically infected animal was killed, and lesions similar to those seen in cattle dying at the same stage of the disease were noticed. In this connection KOCH says: "These experiments explain satisfactorily the widely different statements concerning rinderpest in sheep and goats. Many farmers are of opinion, and this I have seen myself on rinderpest farms in the Free State, that these animals may graze with diseased cattle without contracting rinderpest, while, on the other hand, some have reported that the pest had appeared in their flock of sheep and goats, and carried them off in large numbers, after it had already disappeared among the large stock. My opinion on this subject is that sheep and goats at first contract rinderpest in such a mild form that it can not be diagnosed, and that the pest gradually becomes more virulent through being continuously propagated within these animals' systems. Then the symptoms became naturally more distinct, and in some cases the disease may even take a fatal course."

No other animals than those mentioned were found susceptible to the disease, and birds were particularly exempt from the action of the rinderpest virus.

Chemic attenuation of the virus was unsuccessful; even glycerin, which preserves almost all similar infective materials, destroyed the potency of the virus. Salt solution and distilled water failed to give any protection or immunity. Dessication for a period of four days, rendered the virus innocuous, and failed in protective action.

The most important and the really practical experiments were those relating to the animals recovered from rinderpest, commonly called "salted" cattle. Inoculations with rinderpest blood failed to produce symptoms in the "salted" animals, while the "unsalted" cattle promptly died of genuine rinderpest. The serum of the blood of "salted" cattle was then injected into the susceptible animals. At first, 100 c.c. of serum were used, and on the following day 1.5 c.c. of rinderpest blood was hypodermically injected, without significant symptoms, and on the sixth day there was a reinoculation with negative results. Another animal was inoculated with a mixture of blood and serum without injury. Thus it was difinitely established that the serum of the immune or "salted" cattle possessed a certain protective power, and it is from this fact that the important results will be eventually obtained.

The protective power of the serum from the immune animals is not very great, and probably only lasts a short time, and the quantity of serum necessary for general immunization is too great to render the inoculation on a large scale practicable. However, animals have been immunized to such an extent by a mixture of serum and virulent rinderpest blood as to withstand an injection of 20 c.c. of rinderpest blood, a ten-thousandth part of which is the ordinary fatal dose. Such immunity KOCH judges to be equal to that of the "salted" cattle. KOCH also discovered that the bile of cattle dead from rinderpest, injected under the skin in 10 c.c. doses, is sufficient to render healthy cattle immune by the tenth day at the latest. Four weeks afterward an injection of 40 c.c. of rinderpest blood caused no injurious results.

In concluding his report KOCH says: "Both these above-mentioned facts convince me that rinderpest can be eradicated with but little difficulty, and within a comparatively short time, by putting these methods into practice. The method of immunizing cattle with serum may be used in order to separate from infected areas those tracts of country which are still free from the scourge by means of forming a broad belt between them in which all the cattle are inoculated with the vaccine. The protective properties of the bile will be of inestimable service in infected parts. Nearly every case of rinderpest supplies a greater or lesser quantity of vaccine for those animals which are still healthy."

THE ADRENAL TUMORS GROWING OUT OF THE KIDNEY.

The study of the ductless glands seems to be just making its beginning. The thyroid presents an interesting example, but as yet our knowledge of this organ remains almost as unsatisfactory as complete ignorance. The absence of the thyroid determines a pathologic condition known as myxedema, its abnormal activity another condition known as exophthalmic

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goiter. The former disease has been successfully treated by the implantation of pieces of living thyroid or by feeding fresh or dried thyroid tissue, and the latter has in some cases been cured by the removal of the enlarged or vicious thyroid gland.

An analogous condition prevails in the case of the adrenal glands. When these glands are destroyed by tuberculosis, or by other diseases, a condition known as ADDISON'S disease is instituted. This disease is characterized by a bronzing of the skin, a loss of strength and a rapid, small soft pulse. All the other symptoms of a wasting disease come on and life ends in asthenia. There is however, a condition of great hypertrophy or hyperplasia of the adrenal tissue which shows itself in various degrees of toxicity. In the exaggerated form the patient has a slow pulse of remarkably high tension, a very light colored skin and many of the external appearances of renal sclerosis. These patients however pass normal urine and sometimes die of apoplexy even though quite young. The autopsy reveals adenoid tumor of the kidney resembling the adrenal tissue with or without similar metastatic tumors in the lungs or bones. The lipomas of the kidney so often reported in the literature are, many of them at least, of adrenal origin.

There has been no proper exposition, in our medical literature, of the phenomena of adrenal tumors in the kidney. GRAWITZ first pointed out the origin of these neoplasms (*Langenbeck's Arch.*, vol. 30), and many contributions have been made to the pathologic study by the aid of the physiologic researches of CYBULSKI and SZYMONOWICZ. (*Gazette lekarska*, 1895, XV, and *Pflüger's Archiv*, 1896, LXIV.) These investigators found that the removal of both adrenals produced the small rapid pulse of ADDISON'S disease, and that the injection of the adrenal extract produced a slow pulse of high tension. The injection of the venous blood from the adrenals of one animal into the veins of another, produced the same effect as the adrenal extract. OLIVER and SCHÄFER confirm these observations. Any one may confirm them, in a measure, by using the adrenal extract for its digitalis-like effect.

To the diagnostician and surgeon these phenomena are of the greatest interest. There came under the writer's observation some years ago, a man complaining of his inability to give up the morphin habit. He began to use the drug for pain in the back and severe headaches, which were accompanied by violent palpitation of the heart. After long confinement in a hospital with a gradual diminution of the morphin and the substitution of small doses of atropin combined with careful diet and regular bathing, his health improved, but the pain in his back remained. His headaches were almost constant and only partially controlled by atropin. An examination of his back now disclosed the presence of a tumor just under the last rib and apparently in connection with the kidney. With the patient's consent this kidney was explored through a lumbar incision and found to be attached to the palpable tumor. It was accordingly removed. The man recovered from his pain and headaches and the tumor was found to be an adrenal parathelioma growing out of the middle of the kidney. In another case long taken to be renal sclerosis, the patient was suddenly attacked with symptoms of pneumonia without fever. This was followed by a pleuritic effusion of a bloody color and later by death. The pulse was all the time of the highest tension although the skin was pale. At the autopsy a large adrenal tumor of the kidney was found with multiple metastatic tumors in the right lung.

The size of the tumor does not always determine the virulency of the intoxication. In some cases a small tumor results in such a virulent product that the young sufferer dies of cerebral hemorrhage, while in other cases the tumor reaches a prodigious size without any marked circulatory symptoms.

It is at least remarkable that the increase in the thyroid produces an intoxication, often fatal, with symptoms almost the opposite to those produced by an analogous increase in the adrenals, and that the destruction of the thyroid is accompanied by a remarkable condition (myxedema), the most pronounced symptom of which is related to the skin, while the destruction of the adrenals results in a very different condition (ADDISON'S disease), the most manifest symptom of which is also found in the skin.

These analogies are certainly suggestive, even to the therapeutist, and ought to lead to some rational use of the much abused glandular extracts.

THE SECTION ON PRACTICE.

If nothing had been produced at the Semi-Centennial outside of the papers of this Section, the meeting would still have been of immense value to the American Medical profession. The current number of the JOURNAL containing the important discussion and committee report on serum diagnosis is one that no practitioner can afford to be without, and Dr. MUSSER and his colleagues have set a model that will be very difficult to surpass.

These papers, however, valuable as they are, should serve the additional purpose of further stimulating effort, and realizing to the full extent the wise purpose of the founders. There is much room for original experimentation and it is a matter of congratulation that the subject of prize essays has again been revived with so bright a prospect of ultimate success.

The JOURNAL, as it is now produced, covers each year nearly the whole field of medicine, as the Sections now number twelve.

The already great but growing membership brings that material power which will insure success. Let the whole profession enlist in the ASSOCIATION with

the determination to show the world what the American medical profession can do for science and humanity when united in collective effort.

BRITISH MEDICAL ASSOCIATION—SPECIAL RATES.

The Grand Trunk Railway System will sell reduced rate tickets to all physicians attending the British Medical Association meeting at Montreal, August 31 to September 5. Tickets will be sold August 28, 29 and 30, good returning three days after adjournment of the convention, at rate of one and a third fare for the round trip. Through Pullman palace vestibuled sleepers daily between Chicago and Montreal. For tickets and sleeping car accommodations apply to L. R. MORROW, G. P. & T. A., 103 Clark St., Chicago.

CORRESPONDENCE.

Treatment of Typhoid Fever.—An Answer.

DAVIS, W. VA., Aug. 3, 1897.

To the Editor:—In the JOURNAL of July 10 I notice an article on the "Treatment of Typhoid Fever," by Dr. Upshur of Richmond, Va., and his criticism of Dr. Woodbridge. His treatment especially attracted my attention. He says: "Dr. W. makes claims of such brilliant results that one is startled, and the question arises, have I been groping and blundering all these years?" In the outset he desires it understood that he has never used it, not because of any *spirit of prejudice or unfairness*, but because upon *careful* consideration the brilliant results *claimed*, to *his* mind, disproved the facts, and he did not feel willing to trifle with the lives of his patients by uncertain experiments. He has never tried it. How can we know of anything unless we try it? How does the Doctor do when he hears of a new drug, for instance, antitoxin in the treatment of diphtheria? Did he use it after reading what it did at the hands of others? Does he not use vaccine virus that was hooted at by many for years? Many wise and learned physicians laughed at it and cried it down, but what physician does not now use it, believing firmly in it? It was an experiment at first, but after it was perfected, tried and proven, it became a *fact*. How can he judge of a treatment (no matter what) if he does not give it a trial—a fair trial? Does he practice medicine and never try what others recommend? Does all his practice consist of what he knows most positively are *sure cures?* Can not others teach him? Have we to depend on foreign physicians entirely? Have we not just as wise, just as learned men in our own country as they have across the water? Is it not just as possible for the Woodbridge treatment to succeed as for antitoxin?

I have used Woodbridge treatment exclusively for two years, without the loss of a single case. If I should have typhoid fever I should feel safe, if I took it or had a physician give it who understood just how to use it. I lost from two to five cases every year before I used it. Does Dr. Upshur think the ability to diagnose typhoid fever is confined to himself and a certain few others? He says, "there *must* have been a mistake in diagnosis." I used for years iodin and carbolic acid and cold baths, and had good nurses. I had a case last year who had four hemorrhages from the bowels, temperature ranging from 104.5 to 105.5 degrees, and she got well in three weeks. I used the Woodbridge antiseptic tablets. Her husband was sick with the same disease this summer, between nine and ten weeks, and is really not well yet. He was on the old treatment and under a physician who does not believe in Woodbridge. Since I have been using the Woodbridge antiseptic tablets I have not had a case sick over four weeks, and they rarely run more than three before being discharged. Only the one mentioned had hemorrhage from the bowels and four or five deliriums, which soon passed off. Several cases came to my office with temperature 105 to 105.5 degrees when I first saw them. Do I make mistakes sometimes in diagnosis? Yes, and so do all of us; Dr. Upshur included.

When I see physicians all around me using the old treatment and working and fighting with might and main with this dread disease, I am all the more ready to sound Dr. Woodbridge's praises for his wonderful discovery, and add my voice to the many in urging all physicians to try it and prove its efficacy. I saw one of our physicians lose four cases in one family on the old treatment. The Doctor was discharged and another one called in, who, although he did not use the formula of Woodbridge, used a part of it—thymol, guaiacol carbonate and hydr. chlor. mite, and brought the rest of them through.

Dr. Upshur seems to think that Dr. Woodbridge breaks a law of ethics. I can not see how he does, for if Dr. Upshur would recall how often he puts the make of drug he wants on his prescriptions, he would see in some measure while that is a sort of advertisment for the firm, his motive is to secure what he considers the best make for his patient. I often do this myself, for I have carefully watched and know whose preparations act best for me. Dr. Upshur speaks of the years of experience of the great men past and present, of the convincing truth of their writings, etc. It is true in most instances, but can not others come forward thinking and studying and find out new things and improve on what has already been found out and teach you and me? To sum up: What is our object in this great science of medicine? Is it not to relieve suffering, to cure disease, to get our sick ones strong again, and the sooner the better? I for one intend to read, to study, and to work, but use good common sense with it, and do my very best for my patients. I am heart and soul with Dr. Woodbridge, and firmly believe he has made a great discovery and advise all my brother physicians, who have not already studied and tried it, to do so. Watch it carefully and you can not fail to have your efforts crowned with success. Anyway, do not condemn before giving it a fair trial.

Fraternally, B. M. SMITH, M.D.

Criticism of a Critic.

CLYDE, N. Y., July 28, 1897.

To the Editor:—In the number for July 24, page 196, I notice under the title "The Jubilee Meeting," that among your exchanges there is one journal, *Buffalo Medical and Surgical*, which is deeply grieved that the representative of the anarchistic element of the profession of this State—"president of the Medical Society of the State of New York," was not allowed to participate in the proceedings," etc.

The writer also laments that "a great medical society" was thus ignored.

If the writer was not in his swaddling clothes at the time, does he fail to remember the insurrection in which he and his fellow-revolutionists were engaged an insurrection the purpose of which those revolutionists boldly proclaimed was the expunging of the Code of Ethics from the Constitution of that "great medical society" by which, since 1807, it had been governed?

This mournful state of feeling, mingled with much pity, is so intense that he sorrows because Dr. Davis was not magnanimous enough to allow it, so long as it "would not have diminished the renown that is so justly accorded to the Father of the ASSOCIATION." Herein lies a great mistake of the writer. If he well knew of what he was writing, he knew that Dr. Davis had always protected the Code of Ethics from any and all invasions, and he ought to have known that had the repre-

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sentative of a "new code" been allowed, by vote, "to participate in the proceedings," it would *not* "have been a graceful act." On the contrary, it would have been a *disgraceful* one.

If the revolutionists before referred to desire to "participate in the proceedings" of future meetings of the AMERICAN MEDICAL ASSOCIATION, let them "confess their manifold" errors of the past, come to the *front* door, and they will "receive the right hand of fellowship."

D. COLVIN, M.D.

Carbohydrates in Diabetes.

PARIS, ILL.

To the Editor:—I think Dr. E. L. Munson is right in allowing a moderate amount of carbohydrates in diabetes mellitus. I have done so for the last eight years, and the results have been better than under a strict proteid diet.

I also allow the patient to drink all the water he wants, because it dilutes the urine and makes it less irritable to kidneys and bladder and, I think, lessens the danger of coma.

Respectfully, W. H. TEN BROECK, M.D.

PUBLIC HEALTH.

A Yellow Fever Ship at Cape Charles.—July 30 advices of the arrival of the Norwegian bark *Nor*, with a suspected case of yellow fever have been given out. Two cases developed on the vessel at Kingston, Jamaica. The bark was accordingly sent to the quarantine at Fisherman's Island.

Infectious Disease Hospitals of Massachusetts.—Legislation that will place restrictions upon the building or establishment of homes for consumptives and the like, in towns and cities is sought in that State. The unanimous approval of the boards of aldermen and health, plus that of the mayor, will be prerequisite for a license for all private sanitaria for contagious diseases. What contagious affections are, is thus defined: "The following diseases are infectious and contagious diseases, and diseases dangerous to the public health within the meaning of this act: smallpox, diphtheria, phthisis or pulmonary tuberculosis, membranous croup, measles, scarlet fever, typhoid fever, yellow fever and cholera." The proposed act provides that "no infectious or contagious disease, or other diseases dangerous to the public health, shall be treated or cared for in any city or town, in any hospital, home, or other like institution, not a public hospital maintained at the expense of said city or town, or of the commonwealth, or receiving endowment or assistance therefrom; provided, however, that the board of aldermen of any city, by unanimous vote, with the approval of the mayor and board of health thereof, and the board of selectmen of any town, by unanimous vote, with the approval of the board of health thereof, may license any private hospital, home, or any other like institution, of said city, to treat and care for any of said infectious and contagious diseases, or other diseases dangerous to the public health."

State Supervision of Medical Practice in Rhode Island.—The following are some of the principal regulations governing the issuance of certificates to practice medicine in that State, as adopted by the Board of Health of the State: All applicants applying for a certificate to practice medicine in the State of Rhode Island, presenting a diploma from any foreign medical school must pass an examination before the State Board of Health upon the following subjects: Anatomy, physiology, chemistry, therapeutics, materia medica, surgery, theory and practice of medicine, obstetrics, gynecology, hygiene and State medicine. An average grade of 80 per cent. will be required as conditions of receiving a certificate to practice medicine or surgery in this State. Diplomas from all recognized medical colleges in the United States requiring a minimum of three years study of medicine and two courses of lectures for graduation, prior to 1885, shall be recognized as in good standing, and diplomas issued by the same prior to that date, properly verified, shall entitle the holder thereof to a certificate to practice medicine and surgery in this State. For the ten years ending Jan. 1, 1895, all medical colleges exacting the foregoing requirements, shall, by virtue of such fact, be recognized as in good standing and to include the year 1891, but no medical college shall be recognized as in good standing which has not, since 1891, possessed the foregoing requirements, and which has not, in addition, exacted an entrance qualification and attendance on three regular courses of lectures, no two courses to have been given in any one year, as a condition of graduation. On and after July 1, 1895, no medical college shall be recognized as in good standing which does not require the entrance qualification representing as a minimum a high school diploma, or its equivalent, as a prerequisite for matriculation; which does not possess an adequate equipment for teaching medicine; which has not the clinic and hospital facilities, based upon a minimum municipal population of fifty thousand, and which does not have an active faculty embracing the departments of anatomy, physiology, chemistry, therapeutics, materia medica, surgery, medicine, obstetrics, histology, pathology, bacteriology, ophthalmology, otology, gynecology, laryngology, hygiene and State medicine, and which does not enjoin attendance upon 80 per cent. of four regular courses of instruction, of not less than twenty-six weeks each, in four different years, and which does not exact an average grade of 75 per cent. on examination, as conditions of graduation. A medical college shall be recognized as in good standing, in which the student is conditioned in one or more of the branches as requirements for matriculation, or that admits to advanced standing, students that are matriculants of colleges of pharmacy, or colleges of veterinary medicine, or upon the possession of an academic degree.

Meeting of the State Board of Health of Michigan.—The State Board of Health held its summer meeting in Sand Beach July 30 and 31, 1897, for the purpose of examining plans for State institutions and the transaction of other business. Plans and specifications were examined and recommendations made for a hospital cottage for female patients at Michigan Asylum, Kalamazoo. Plans and specifications were examined and recommendations made for a hospital building and also for a work shop at the State School for the Deaf at Flint.

President Wells congratulated the Board on the fact that at the recent session of the legislature a small appropriation was made which enables the Board to fulfil the Act of 1895 relative to teaching in the schools how to restrict the most dangerous communicable diseases; he suggested that the United States life saving station at this place may be examined by the members of the Board and mentioned in this connection the work of the Board relative to the resuscitation of the drowned. The plan recommended by the Michigan Board necessitates the action of only one person, whereas other methods necessitate action by two. At this meeting Secretary Baker presented a paper on the treatment of the drowned, suffocated and electrically shocked, to be published in the annual report. The Board's leaflet on the treatment of the drowned was slightly amended, a new edition ordered printed and copies to be distributed to life saving stations, lake summer resorts and other places around the State. The leaflet includes rules for efforts at resuscitating persons electrically shocked.

Dr. Belknap brought up the request of a local health officer that the State Board supply blanks for the use of householders and physicians in reporting contagious diseases. Secretary Baker pointed out the fact that the appropriation was not sufficient to enable the State Board to print so many blanks as would be required for that purpose; under such circumstances he claimed it was the duty of the various local boards of health to supply those blanks.

Professor Fall brought up the subject of the collection of samples of water from different parts of the State to be tested for chlorin in order to map out the normal amount of chlorin in the natural waters about the State to enable analysts to

judge of the safety of samples of water hereafter examined, with a view to their use as water supplies. Professor Davis of Alma College, who is working on the State geological survey, is to collect samples of water in the eastern part of the State and send them to Professor Fall for examination. Professor Fall's map relative to chlorin in natural waters relates thus far mainly to the southern part of the State. He is now preparing to have samples of water sent by mail, more cheaply than heretofore by express.

The subject of quarter-centennial celebration of the establishment of the Michigan State Board of Health, in Detroit, July 30, 1898, was considered. Special subjects were referred to the several members of the Board who were charged with the duty of preparing for presentation at that meeting papers on a number of important subjects. The Secretary was directed to issue to the several ex-presidents and ex-members of the Board formal invitations to be present and take part in the quarter centennial celebration. The President and Secretary of the Board were appointed a committee to attend the meeting of the National Conference of State Boards of Health in Nashville, Tenn., Aug. 18 and 19, 1897. The two delegates were authorized to vote at that meeting for the proposed amendment of the rules for the transportation of corpses, the design being to have such rules uniform throughout the United States and adjoining provinces.

Dr. Cattermole of the office of the Board, was asked to give special study to the subject of the best measures for local health officers to take in connection with the State Laboratory of Hygiene, for the bacteriologic diagnosis of the dangerous communicable diseases.

The subject of the alleged pollution of the St. Clair River was brought up by a letter from a health officer of a township situated on that river, who claimed that there is "a large and increasing amount of sewage in the waters of the river St. Clair, produced by the discharge of the sewers of Port Huron and St. Clair cities into said river above this point." He suggests "that such oversight be used by the State Board of Health, as would prevent an accumulation of sewage matter in all the rivers of this State to such an extent as shall be dangerous to the public health." Secretary Baker mentioned a paper read at the last meeting of the Michigan Society of Engineers, by Engineer Williams of the Detroit Water Board Office, pointing out the facts relative to the unusual occurrence of typhoid fever in Detroit immediately following the dredging of sewage sludge from the bottom of Black River, Port Huron, and the emptying of that sludge into the St. Clair River. Secretary Baker read a communication from the Secretary of the Massachusetts State Board of Health, in reply to his letter of inquiry, which says: "In regard to the general act in force in this State for the protection of the purity of inland waters, I can say without hesitation, that the Act as amended in 1888 is one of the most important and valuable laws ever enacted in this State. It has already accomplished a great deal for the purification of public water supplies and the adoption of efficient methods of sewage disposal in cities and towns, and has undoubtedly saved to the State a far greater amount, financially, than has ever been appropriated for the purpose of the Act. Many towns and cities would, undoubtedly, have adopted unwise and expensive schemes had it not been for the enactment of this law." "The law was amended in 1888 . . . and appears to be at present as perfect a law as can be asked for." The subjects of sewage filtration laws and the protection of the purity of the inland waters of the State, was referred to the two Committees of the Board on Water-supply and Sewerage, Professor Fall to act as chairman of the joint committee.

A letter from Samuel G. Milner, M.D., member of the Board, not present at this meeting, proposed several subjects for action, including the prevention of infection through milk supplies, the inspection of sewage and other sanitary arrangements at Michigan health resorts, the isolation of tuberculous inmates in asylums and poorhouses, all of which subjects seemed to call for further legislation, and were therefore referred to Judge McAlvay, the committee on legislation.

Professor Fall reported the result of his work in the preparation of a pamphlet for the use of teachers in complying with the law which requires the teaching in the public schools of the best measures for the prevention of the most important diseases. Several hours were devoted to the examination of the work, and in the discussion of the subject. The Secretary was directed to have copies made, and one sent to each member of the Board for further study, with the view of perfecting it for publication.

A vote of thanks was extended to Mr. Wells for his entertainment of the members of the Board during this meeting and for the courtesies extended to them during their stay in Sand Beach.

BOOK NOTICES.

The American Text-book of Operative Dentistry. In contributions by eminent American authorities. Edited by EDWARD C. KIRK, D.D.S., Professor of Clinical Dentistry, University of Pennsylvania, Department of Dentistry. In one very handsome octavo volume of 699 pages, with 751 engravings. Cloth, $5.50; leather, $6.50, net. Philadelphia and New York: Lea Brothers & Co.

The contributors to this volume are R. R. Andrews, Henry H. Burchard, Calvin S. Case, William E. Christensen, Dwight M. Clapp, M. H. Cryer Edwin T. Darby, C. L. Goddard, S. H. Guilford, Louis Jack, Edward C. Kirk, Louis Ottofy, C. N. Peirce, J. D. Thomas, Alton Howard Thomas.

This book is a successful attempt to bring modern pathology to bear upon the old traditions and has resulted in an essentially modern book, retaining the essence of the past literature which was of value, so arranged and presented as to meet the requirements of those for whom it was written. From a literary standpoint the work is a distinct advance on the text-books of the past, and will add to the reputation of the dental profession in this line.

The surgical anatomy of the teeth and jaws is correctly given in every detail, and the subject of implantation of teeth receives careful attention. Dr. Younger is given the credit justly due him of the authorship of modern implantation. The chapter on malformation is especially good. In mechanical dentistry the work will be found fully up to date. As a whole, we have unstinted praise for this work.

Reference Book of Practical Therapeutics. By various authors, edited by FRANK P. FOSTER, M.D., editor of the *New York Medical Journal*. Vol. 2, pp. 618. New York: D. Appleton and Company. 1897.

The following are the contributors to the volume: Samuel Treat Armstrong, M.D., Ph.D.; Samuel M. Brickner, M.D., William B. Coley, M.D.; Floyd M. Crandall, Jeremiah T. Eskridge, Matthias Ganckton Foster, A. G. Gerster, M.D., Henry A. Griffin, M.D., Charles Jewett, A.M., M.D., Howard Lilienthal, M.D., Russel H. Nevins, M.D., Austin O'Malley, M.D., George L. Peabody, M.D., Frederick Peterson, M.D., Samuel O. L. Potter, A.M., M.D., Charles Rice, M.D., Solomon Solis-Cohen, M.D., James T. Whittaker, M.D.

This volume which is fully equal to its predecessor is arranged alphabetically and will be found of highest value for reference especially in relation to newer remedies, mineral springs and mineral waters. The index is very full and adds much to the value of the work.

We congratulate the distinguished editor on the completion of the work, which for convenience and completeness is unexcelled.

Eye-strain in Health and Disease. With Special Reference to the Amelioration or Cure of Chronic Nervous Derangements without the aid of Drugs.—By AMBROSE L. RANNEY, A.M., M.D., author of "Lectures on Nervous Diseases." "The Applied Anatomy of the Nervous System," etc., etc.; late Professor of Nervous Diseases in the Medical Department of the University of Vermont and of the Anatomy of the Nervous System in the New York Post-Graduate Medical School, etc. Illustrated with 38 wood-cuts. One volume, royal octavo, pages viii-321. Extra cloth, beveled edges, $2,00 net. The F. A. Davis & Co., publishers, 1914 and 1916 Cherry Street, Philadelphia; 117 W. Forty-second Street, New York; 9 Lakeside Building, Chicago.

The author of this volume is an enthusiast in the eye treatment of chorea and epilepsy and backs up his opinion with the recital of cases apparently cured by the correction of the eye defects. It must be conceded in the present inaccurate state of the treatment of epilepsy that it is well in all cases for the practitioner to consider the relation of eye strain to the particular case under observation and there is no question but in many cases the correction of visual defects will be found of great advantage.

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